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CLIMATOLOGY OF TINAJAS ALTAS, ARIZONA:[1] PRELIMINARY REPORT.

THE STATION.

TINAJAS ALTAS (colloquially, Tinaxaltas; a Spanish term commonly rendered 'high tanks') may be described as a locality in extreme southwestern Arizona about forty miles east of the mouth of Rio Colorado and three miles north of the Mexican boundary. The name denotes a number of tinajas, or tanks ('water-pockets' in the vernacular of western America, 'pot-holes' in British geology), found in the bottom of a precipitous gorge on the northeastern side of Sierra Gila, a granite range extending southeastward from Gila City and terminating in Sonora a few miles south of the international boundary. The tanks were well known to the aborigines in prehistoric times, and have been known to white men since Padre Kino passed by them in that notable expedition of 1699–1701 which led to the rediscovery of Rio Colorado and the subsequent mapping of the country beyond as a peninsula rather than an island. While California was a province of Mexico, one of the main overland routes connecting it with the capital

[1] Published by permission of the chief of the U. S. Weather Bureau.

city touched Tinajas Altas as the sole permanent 'water' in the 125 hard miles between Rio Sonoyta and Rio Colorado—*i. e.*, in 'El Camino del Diablo' which in the more fertile valleys of California became 'El Camino Real'—and ever since the supply has been deemed unfailing; though singularly enough the water was practically exhausted in the summer following the exceptionally wet winter and spring of 1905, seemingly for the reason that the unusually prolonged and gentle precipitation served rather to fill the tinajas with sand than to sweep them clean and leave them brimming, as do the ordinary rains of the region.

By trail the locality is 75 miles southeast of Yuma, by air-line 15 or 20 miles less; it is 35 or 40 miles south of Gila River at its southerly bend east of Gila City, and 40 or 50 miles north of the nearest shores of Gulf of California; the closest habitations are at the Southern Pacific station Wellton, some 30 miles away in an air-line, and more by any practicable route—there is no road.

The altitude of the lowest tinaja and the adjacent mesa on which the station was established is about 1,400 feet; the contiguous gorge-walls and peaks rise precipitously 750 feet, and some of the neighboring crests 2,000 feet higher (the United States and Mexican boundary survey maps locate near-by peaks above the 800-meter contour line). Albeit a low range, Sierra Gila is notably rugged and conspicuously sterile—it is, indeed, one of the most utterly barren mountain masses in America. From its western base a sandy plain, broken by a few low buttes and ranges, stretches southwestward to the Cocopa Mountains far beyond the Colorado, inclining southward to the gulf; east of it lies a smooth valley-plain seven to twelve miles wide, bounded beyond by a parallel granite range—Sierra de la Cabeza Prieta. Still further eastward lie other more or less parallel ranges with intervening valleys; the Mohawk Range, Sierra Pintada, Sierra Pinecate (on the Sonora side), Ajo Mountain, Sierra Quijotoa and the Baboquivari being the more conspicuous elevations about which summer clouds are wont to gather.

The station equipment comprised (1) a weather bureau instrument shelter of the type used by 'volunteer observers' (mounted on a base about four feet high, improvised from the packing-crate and grub-boxes and guyed with baling-wire); (2) maximum and minimum thermometers; (3) a sling psychrometer; (4) a barograph (supplemented by a fine aneroid of French make); and (5) an improvised rain gauge.

OBSERVATIONS AND GENERALIZATIONS.

Observations were made and recorded twice daily, viz., 8 A.M. and 8 P.M. local time; the hour of observation being advanced some minutes (in accordance with suggestions growing out of hourly observations during the earlier days) to counterbalance the effect of the local topography: for the station occupied a deep gorge opening east-northeastward in which the rays of the morning sun were concentrated by reflection, while the afternoon sun set behind Sierra Gila at 4 to 4:15 P.M. The outfit reached the ground and the instruments were unpacked during the afternoon of May 20, and experimental observation began on the morning of May 21 and was well under way on May 23;[2] the observations were continued until after sunset August 28, when the station was hastily

[2] In the transcripts of the records and averages first made, the period of systematic observation was reckoned as beginning with May 23; but on finding that the total period of observation was exactly one hundred days reckoned from and including May 21, the experimental record was afterward incorporated.

dismantled and the equipment transferred during the night to Wellton *en route* to Yuma.

The observations comprised pressure (determined by barograph and aneroid), temperature, humidity, wind, precipitation and cloudiness.

The barograph was used in accordance with a courteous suggestion by Sumner Hackett, U. S. weather observer at Yuma, in the hope that its record might throw light in the persistent lows of the California Gulf region. This record has not yet been compared and discussed.

The wind observations were rendered practically worthless by local conditions; the atmosphere in the gorge was habitually disturbed by dust whirls drifting in from the plain with intensity rapidly increasing as they caught the rock-warmed air-volume between the canyon walls, so that even the pressure was rendered unsteady (in one case a single whirl of momentary duration caused the barograph pen to record a change in pressure of over twenty feet); while the arrangement of neighboring peaks and precipices so disturbed the general air currents that their direction could seldom be determined confidently without a journey of two or three miles eastward on the plain, or a three-hours' climb up the granite precipices and peaks.

From the beginning of experimental observation on May 21, the temperature records (both of the maximum and minimum and of the wet and dry bulb thermometers) seemed notable by reason of a relatively low diurnal range—a range considerably less than that previously noted by the station agent in connection with geologic and ethnologic work in Nevada, Sonora and southern-central Arizona. The minitude of the range, indeed, awakened a suspicion of inaccurate reading and led to the above-noted delay of two days in beginning the more formal record transmitted to the observer at Yuma; it also led to a series of experiments with coverings for the wet bulb of the psychrometer, resulting in the adoption of a somewhat thicker and more spongy covering and a longer period of swinging than more humid climates require. Before the middle of June, however, the record developed such consistency as to establish the general accuracy of the observations. The accompanying Table I., transcribed from the original field-sheets, illustrates the lowness of diurnal and general ranges in temperature and also in moisture—including rain as well as cloudiness (expressed in tenths of total sky estimated for the visible and inferred for the invisible portions)—during the one hundred days from May 21 to August 28. As shown by this table, the extreme thermometric range during this period was but 62.8°, the range

TABLE I.—TEMPERATURE AND MOISTURE EXTREMES.

Term.		Temperature.			Moisture.							
		Self-reg. Therm's.			Dew-point.			Relative Humidity.			Rain (in.).	Cloudiness.
		Max.	Min.	Range.	Max.	Min.	Range.	Max.	Min.	Range.		
May (21–31)	A. M.	88.4°	54.6°	33.8°	54.0°	42.0°	12.0°	62%	25%	37%	0.00	7
	P. M.	96.0	54.6	41.4	48.0	35.5	12.5	56	18	38	.00	2
June	A. M.	97.4	65.4	32.0	52.0	25.0	27.0	37	10.5	26.5	.00	7
	P. M.	105.9	77.1	28.8	52.0	17.0	35.0	32	6	26	.00	0+
July	A. M.	109.0	73.6	35.4	68.5	21.0	47.5	63.5	6	57.5	T	8
	P. M.	117.4	79.7	37.7	62.0	12.0	50.0	54	4	50	T	3
Aug. (1–28)	A. M.	102.0	73.9	28.1	72.0	29.0	43.0	75	8	67	.12	10
	P. M.	110.2	77.4	32.8	66.0	40.0	26.0	48	12	36	.00	8
Extreme ranges.				62.8			60.0			71		

in dew-point only 60°, and that in relative humidity only 71 per cent.—ranges not infrequently exceeded within twenty-four hours in certain desert localities.

As the record developed, establishing a persistently low diurnal range in temperature with consistent values for moisture, special attention was turned to the monthly means. Semi-daily means (including rain and also cloudiness expressed in tenths and fractions) are given in the accompanying Table II. As shown by this table, the approximate mean diurnal ranges in temperature varied from 11.4° to 15.9°, the mean depression of the wet-bulb thermometer from 15.2° to 29.2°, and the relative humidity from 15.9 per cent. to 40.6 per cent.; ranges suggesting a moderately humid rather than a distinctively arid climate.

Even more striking than the semi-diurnal means are the daily means summarized by months; largely transcribed from the monthly records, these are recapitulated in Table III. As indicated by the table, the mean diurnal range for the one-hundred-day period varied between 20.1° and 24.5°; and it may be noted that the greatest and least daily ranges in June were 30.4° and 13.4°, the absolute range for the month being 40.5°, while the corresponding values for July were, respectively, 28.0°, 13.9° and 43.8°, and for August 25.0°, 13.0° and 36.3°. The psychrometric and humidity means are consistent, and the ranges correspondingly low. (The average dew-point and relative humidity computed from the means in the table are, of course, not strictly accurate; the values corresponding to the means of 88.9° for the dry-bulb and 64.5° for the wet-bulb thermometer would be respectively 49° and 25 per cent.)

Although deemed of only moderate value, the records of precipitation and

TABLE II.—TEMPERATURE AND MOISTURE MEANS.

Term.		Temperature.						Moisture.			
		Self-Reg. Therm's.			Psychrometer.			Humidity.		Rain (in.).	Cloudiness.
		Max.	Min.	Range.[3]	Dry Bl.	Wet Bl.	Deprsn.	Dewp't.	Rel. H.		
May (21–31)	A. M.	**78.5°**	**64.4°**	14.1°	**73.4°**	**58.2°**	**15.2°**	46.8°	**40.6%**	0.00	1.2
	P. M.	86.2	70.5	15.7	76.0	59.0	17.0	45.5	36.6	.00	.5
June	A. M.	90.6	74.7	**15.9**	83.8	59.4	24.4	38.8	21.6	.00	.4
	P. M.	98.9	86.1	12.8	89.6	60.4	**29.2**	**34.4**	**15.9**	.00	.4
July	A. M.	94.8	80.0	14.8	90.5	66.4	24.1	49.1	29.1	T	1.0
	P. M.	**102.2**	90.1	12.1	93.8	66.0	27.8	45.9	22.6	T	.4
Aug. (1–28)	A. M.	94.6	81.7	12.9	90.7	**69.8**	20.9	**57.3**	37.3	0.04	1.0
	P. M.	101.8	**90.4**	**11.4**	**94.0**	69.6	24.4	54.8	28.5	.00	1.2

TABLE III.—TEMPERATURE AND MOISTURE SUMMARIES AND AVERAGES.

Term.	Days.	Temperature							Moisture.			
		Self-registering Therm's.				Psychrometer.			Humidity.		Rain (in.).	Cloudiness.[5]
		Max.	Min.	Range.	Mean.	Dry Bl.	Wet Bl.	Depr.[4]	Dewp.	Rel. H.		
May (21–31).	11	**86.2°**	**64.1°**	22.1°	**75.1°**	**74.7°**	**58.6°**	**16.1°**	44.8°	**38.6%**	0.00	0.6
June.	30	98.9	74.4	**24.5**	86.6	86.7	59.9	**26.8**	**36.6**	**18.5**	.00	.23
July.	31	**102.2**	80.0	22.2	91.1	92.1	66.2	25.9	47.5	25.9	T	.7
Aug. (1–28).	28	101.8	**81.7**	**20.1**	**91.7**	**92.3**	**69.7**	22.6	**56.0**	32.9	.002	1.2
Averages for	100	99.3	77.0	22.3	88.2	88.9	64.5		46.5	27.1	.001	.67

[3] Approximate values taken from means in this table.

[4] Approximate values taken from means in this table.

[5] Taken from monthly summaries.

cloudiness are consistent with those of temperature, and are hence of some significance. The total precipitation of the period comprised 0.11 inch on the morning of August 9 and 0.01 inch on the morning of August 27, as measured at the camp within a few yards of the instrument shelter. On both occasions the amount varied greatly in both vertical and horizontal directions, the zones of precipitation being narrow and the greater part of the initial volume being evaporated in the lower atmospheric strata—indeed, but a small part of the rain precipitated from the dark bases of the cumulus clouds ever reached the ground. In the heavier precipitation of August 9 the rain diminished to a trace within 300 yards down the arroyo and some 60 feet below the level of the station, while on the gorge walls 300 feet south of and 200 feet above the station the fall was probably between 0.20 and 0.30 inch.[6]

The value of the cloud observations was impaired by the smallness of the visible sky, only about 50 per cent. of the total being in sight at the station. From E. to N. 30° E. the horizon was broken only by Sierra de la Cabeza Prieta some ten miles away, and the still remoter Mohawk Range; thence by north to west the skyline averaged some 20° above the horizontal, while the mean altitude of the southerly sky line was over 30°. Accordingly, stratus and other clouds toward the western and southern horizons were seldom seen. Some 90 per cent. of the clouds observed gathered or drifted in northerly directions over Cabeza Prieta and other ranges east of the station; and but few cirrus and still fewer cumulus clouds formed or floated over Tinajas Altas and the neighboring portions of Sierra Gila. In general the cloudiness diminished somewhat from the middle of May to near the end of June and then increased during July and August (as indicated in Table II.); in general, too, the cloudiness was less in the evening during the earlier part of the period, with a tendency toward becoming greater in the evening in the later part. Concurrently with the cloud and rain observations a record was kept of thunder, lightning and haze, which were lacking in May and June, fairly frequent in later July, and quite frequent in August—indeed during the later half of August more or less distant thunder was heard almost every afternoon and sheet or flash lightning was seen during most evenings. By far the greater portion of the located thunder and over 75 per cent. of the visible lightning occurred in the east, *i. e.*, over Cabeza Prieta and other ranges; and at least a dozen—perhaps a score—of rainfalls were seen in the same quarter. Occasional views were had of the entire horizon from the crests and peaks of Sierra Gila; and while clouds were sometimes seen over the valley of Rio Colorado and the Cocopa Mountains beyond, these only confirmed the observations made at the station which served to locate a zone of precipitation passing in a northerly direction over Cabeza Prieta and neighboring ranges, *i. e.*, from some ten to fifty or one hundred miles further eastward than Tinajas Altas and Sierra Gila. There were no observations on dew, for the sufficient reason that throughout the entire period there was none; the greatest relative humidity at the hours of observation was but 75 per cent. and the maximum probably fell below 90 per cent. even in the feeble rains—during which it was evident that evaporation from

[6] This rainfall, especially on the rocks above the level of the station, served to partly refill the tinajas and so prolong the maintenance of the station; the evening before its occurrence the total quantity of water remaining was estimated at five gallons, or four days' supply, with an extra gallon for the walk to Wellton.

fabrics, etc., proceeded nearly as rapidly as wetting. Similarly, no observations were recorded on mirages or other phenomena of refraction, of which there was a notable dearth; on a few occasions of light dust daze due to strong winds over the crests and broader valleys, Sierra de la Cabeza Prieta appeared magnified and brought near, though its angular altitude (determined by sighting over fixed points in rocks adjacent to the instrument shelter) did not vary perceptibly; and during the entire one hundred days not a single mirage of the type so common a few score miles further eastward was seen. In a word, the atmospheric phenomena in general, like the temperature records, denoted a condition of striking steadiness or stability during the period of observation.

Tinajas Altas was chosen primarily as a site for a study of light and its effects on desert organisms; and after unsuccessful efforts to obtain a satisfactory photometer a series of determinations of the intensity of light by means of a simple exposure meter was planned. The record is of little value, partly because the paper used in the apparatus is sensitive to moisture as well as light, but in the inverse direction, so that the time of coloration gave merely an inseparable measure of aridity and luminosity. In general the record indicates a light intensity twice or thrice that of Saint Louis—a measure probably worth less than the common experience of photographers that in the arid region given plates ordinarily require but one half to one fifth of the exposure needed in humid regions.

COMPARISONS.

Tinajas Altas was selected as a desert station, yet the records were found notably discordant with incidental recollections of and notes on climate in the more desert districts of Nevada, southern-central Arizona and central Sonora. The absolute and mean maximum temperatures were decidedly lower and the diurnal thermometric range much lower, while the mean temperature was rather higher and the absolute and mean minima much higher than anticipated; concordantly, the moisture (dew-point and relative humidity) was both higher and much less variable than previous experiences indicated as probable or even possible—it is, indeed, a striking fact that the entire range of relative humidity during a period of one hundred days should be no more than 71 per cent., and it is even more striking that, despite the considerable average humidity, the dew-point was never reached during the one hundred-day period. In short, while Sierra Gila and the adjacent plains are typical if not utter deserts, the climate of Tinajas Altas impresses the observer as subhumid rather than arid—and this despite the limited precipitation.

As the record assumed form during July and August, definite comparisons were made with other stations in Arizona, especially with the cooperative (until recently known as volunteer) stations in the western division of the territory; this being made easy through the monthly records issued by the 'Arizona section of the climate and crop service of the Weather Bureau' published by authority of the Secretary of Agriculture, under the direction of the chief of the Weather Bureau, by Lewis N. Jesunofsky, section director. Contrary to a widespread impression (first produced when Fort Yuma was the sole station in its region), these reports show that Yuma is (at least during summer) by no means the hottest station in the country—indeed, with the single exception of the mountain station Kingman, its temperature is both lower and more equable than that of any other station in western Arizona: Aztec, Mohave, Mohawk, Parker and Sentinel, with several other points in the southern

division, giving higher extremes, ranges and means. The temperature records of southwestern Arizona (including Tinajas Altas) during the summer of 1905, in fact, fall into three groups or classes, viz: (1) the mountain class, represented by Kingman; (2) the interior plains class, comprising Aztec, Mohave, Mohawk, Parker and Sentinel; and (3) what may be called the Gulf class, represented by Yuma and Tinajas Altas. Denoted by their more striking features, these may be considered, respectively, the cool, the hot and the equable varieties or phases of southwestern Arizona's summer climate. These are illustrated in the accompanying Table IV., compiled chiefly from Jesunofsky's report for July;[11] two typical stations of the southern division being introduced partly to illustrate the extension eastward of the two interior classes. The classes of climate or their districts might be defined also by moisture (vapor, precipitation, or both) though they intergrade; in general it may be said—though the qualifying factors are many—that the vapor-content decreases and the precipitation increases east and northeast of the gulfward zone represented by Yuma and Tinajas Altas.

TABLE IV.—CLIMATAL TYPES OF WESTERN ARIZONA. (Defined by Temperature Records for July, 1905.)

Station.		Altitude	Extremes.				Mean.			
			Highest.	Lowest.	Max. Range.		Maximum.	Minimum	Range.	Monthly Mean.
					Day.	Month.				
Interior Plains Class.	Aztec.	492	125.0°	65.0°	51.0°	60.0°	117.9°	74.50	43.4°	96.2°
	Ft. Mohave.	604	124.0	65.0	52.0	59.0	117.7	73.3	38.4	92.5
	Mohawk.	538	126.0	85.0	[7]		[7]	[7]		100.3
	Parker.	345	127.0	61.0	59.0	66.0	111.9	69.5	42.4	90.7
	Sentinel.	685	122.0	82.0	[7]		[7]	[7]		94.5
	Gilabend.[8]	737	120.0	68.0	48.0	52.0	110.5	73.1	37.4	91.8
	Averages.[9]	567	124.0	64.8[10]	52.5	59.2	113.0	72.6	40.4	94.3
Mountain Class.	Kingman.	3326	109.0	53.0	47.0	56.0	99.1	65.8	33.3	82.4
	Congress.[8]	3688	109.0	67.0	25.0	42.0	98.1	77.5	20.6	87.8
	Averages.[9]	3507	109.0	60.0	36.0	49.0	98.6	71.7	27.0	85.1
Gulf Class.	Yuma.	141	116.0	66.0	41.0	50.0	104.4	74.0	30.4	89.2
	Tinajas Altas.	1400	117.4	73.6	28.0	43.8	102.2	80.0	22.5	91.1
	Averages.[9]	770	116.7	69.8	34.5	46.9	103.3	77.0	26.3	90.6

[7] Not given in Jesunofsky's tables.

[8] Introduced from adjacent 'division' for comparison.

[9] Computed from Jesunofsky's figures (excepting Tinajas Altas).

[10] Excluding Mohawk and Sentinel records, in which actual minima do not appear.

[11] Published at Phœnix, August 22, 1905. Other monthly reports are entirely consistent, though not accessible at this writing.

GENERAL INFERENCES.

Through earlier observations in Arizona, Sonora and California the coastwise zone commonly called the fog-belt—an irregular zone extending inland from three to twenty-five miles (according to local configuration) from the Pacific and Gulf coasts—was well known, and its dominant influence on flora and fauna was understood; and the recognition of this zone led naturally to inferences concerning the more striking climatic features of Tinajas Altas and Yuma. So the records were soon seen to indicate the existence of a zone parallel with and inland from the fog belt which may be both denoted and described as a vapor belt, *i. e.*, a zone in which the aggregate volume of aqueous vapor is con-

siderable, although by reason of the prevailing high temperature it remains far above dew-point and so persists in notably stable condition. The volume of vapor is, of course, due to the proximity of the Pacific and the Gulf whence it is derived; its stability is due to the much higher temperature of the land surfaces over which it is carried by the prevailing air-drift, and the consequent warming of the air and rise of the dew-point. Of course the zone is atmospheric rather than terrestrial, meteorologic rather than geographic, and variable in position and character, with many factors of both configuration and climate; in some measure it grades into the fog-belt in both space and time; yet the observations, incomplete as they are, indicate that the vapor zone paralleling the Pacific coast is a definite physical entity.

On considering the relations of the vapor zone to the Californian Gulf, it was at once seen that the general climate and configuration of the region must tend greatly to increase its extent and enlarge its efficiency as a factor affecting what may be called the continental climate of the interior. During the summer the prevailing wind-drift over the region is eastward and northeastward, *i. e.*, from the open Pacific over the prominent backbone of the peninsula of Lower California and thence over the generally warmer waters of the enormously long trough forming the Californian Gulf, so that the volume of vapor would naturally exceed that drifting inland over an unbroken shore-line; then, once within the great sub-aerial trough, this vapor may not easily escape backward against, or in any other direction than with, the prevailing air-currents; it may not even flow down the trough to its open mouth 500 to 1,000 miles away, by reason of the great distance and the internal friction of vapor and air; hence it must form a vast reservoir of vapor overflowing (occasionally or steadily as conditions may determine) northeastward. The effective capacity of this reservoir must be greatly increased by the prevailing character of the land surface, of which from 50 per cent. to far more than 90 per cent. is bare white sand or rock from which heat passes rapidly and constantly by reflection and radiation and convection into the atmosphere, so that the air is highly heated and the vapor firmly fixed within it; it must be increased also by the augmented diathermancy of the atmosphere and the resulting intensity of insolation on the land surface attending the relative drying of the atmospheric mass with its rise in temperature; the effective capacity must be still further increased by the barrier forming the western rim of the trough (the sierras making up the backbone of the Californian peninsula) which tends to retard and check the westward-moving atmospheric wave marking the semi-diurnal increase in pressure consequent on morning insolation. These factors culminate in summer when insolation is strongest; and they would seem adequate to account for the persistent daily summer pressures in the gulf-trough which have already attracted the attention of climatologists.[12] In short, the preliminary survey of the climatology of southwestern Arizona indicates that just as the Californian Gulf is one of the world's greatest tide producers (its spring tides increasing from little more than two

[12] The barograph was carried from Yuma to Tinajas Altas at the courteous instance of Observer Hackett and with the approval of Chief Moore in the hope of obtaining better light on the character and distribution of these summer pressures, and it is probable that when the records are discussed they will be found to corroborate the inferences from temperature and moisture observations; although on looking backward over the summer's work it is easy to see that the barograph records would be much more valuable had another instrument been kept at Yuma for comparison.

feet at its mouth to twenty-five feet at its head), so its aerial portion forms an effective mechanism for promoting the flow and retarding the ebb of the atmospheric tide; and it may be noted that just as the oceanic tides produce astoundingly turbulent currents in the gulf waters mid-length of its trough, so the air-vapor tide results in the astonishingly sudden wind storms and strong williwaws of Tiburon Island and the adjacent coasts.

The extent of this vapor reservoir must be large, though variable from season to season (greatest in summer, least in winter), and from day to day according to a variety of factors and conditions which—like those of climatology in general—are commonly cumulative in effect. The coastward boundary of the vapor zone must shift with the conditions governing the presence or absence of fog, etc.; while the inland boundary must be determined from place to place by altitude, local configuration, presence or absence of floral covering, and other factors influencing temperature, dew-point, precipitation and storm centers. Tinajas Altas and Yuma would seem to lie in the vapor belt or reservoir, while Mohawk, Parker and other stations in the western division of Arizona would seem to lie without it. Proceeding from these points, with the guidance of the July and August storms in the ranges east of Sierra Gila, the summer inland margin of the vapor belt or reservoir may be drawn southeastward from Rio Colorado at a point twenty-five miles above Yuma to Rio Gila ten miles east of Gila City, and thence to and along Sierra de la Cabeza Prieta, crossing the international boundary near the eastern line of Yuma County; and thence more nearly southward and roughly parallel with the gulf coast to Hermosillo (Sonora) and on—gradually approaching the gulf—to regions beyond the influence of this great geographic trough: though it may be noted that the general vapor belt (apparently so greatly expanded about the Californian Gulf) must parallel much of the Pacific coast wherever configuration favors, perhaps attaining next best development in western Peru, where broad Piedmont plains of distinctively desert climate intervene between the Andes and the coast. Within the expansion of the Californian trough the vapor belt would seem especially potent in its influence on continental climate, through the development of storm centers if not otherwise—indeed, a considerable proportion of the effective storm centers traced in their course across the country during recent years, through the observations of the Weather Bureau, originate in this region.

Incidentally, the observations at Tinajas Altas throw light on a question suggested by earlier notes on the climate of Arizona and Sonora, *i. e.*, the question as to relation between what may be called the California type of climate, characterized by a winter rainy season and a summer dry season, and the Sonora type, comprising a winter rainy season and a summer rainy season, with intervening dry seasons; for in the light of the Tinajas Altas records it would appear that the types correspond, except that in the Sonoran region the effect of the vapor reservoir above the gulf in summer (when the land surfaces are hottest) is to develop storms drifting into the interior from the zone in which the vapor is for a time imprisoned.

A CONSTRUCTIVE SUGGESTION.

If the foregoing inferences are valid, it is manifest that the conditions existing about the Californian Gulf exercise important influence on the climate of the continent, and are worthy of correspondingly careful consideration. Any investigation might properly begin with observations made at a few suitably located stations,

which, fortunately, may now be found; these should include either one or both of the new towns west of Yuma, Calexico and Mexicala, together with Caborca, the westernmost town in Altar District, Sonora (to which the Mexican government telegraph line has recently been extended), and Hermosillo, the capital of Sonora. Doubtless cooperative stations might be established at these points; at the latter two perhaps in accordance with the law authorizing observations at points outside of the United States on Gulf of Mexico and Caribbean Sea. It would be well, too, to establish a record station on Rio Colorado between Yuma and Parker, and if practicable also at Quitobaquito (or Humboldt post-office) on the international boundary, some ninety miles east of Tinajas Altas. Records at these points would serve both to determine the limits and define the nature and influence of the vapor zone above the Californian Gulf.

W J McGee.

A SYMPOSIUM ON CHEMISTRY REQUIREMENTS.[1]

That charming and discerning essayist of to-day, Samuel M. Crothers, in a recent issue of *The Atlantic Monthly,* calls our attention to the delightful ease with which one may cultivate a good crop of fallacies by employing a heavy mulch of statistics. "The best way," he suggests, "is to prepare circulars containing half a dozen irrelevant questions, which you send to several thousand persons—the more the better. If you enclose stamps, those who are good-natured and conscientious will send you such odd bits of opinion as they have no other use for, and are willing to contribute to the cause of science. When the contributions are received, assort them, putting those that strike you as more or less alike in long straight rows. Another way, which is more fanciful, is that of arranging them in curves. This is called 'tabulating the results.' When the results have been thoroughly tabulated, use them in the manner I have described for the protection of your favorite arguments."

And yet it is less Crothers that speaks than his Scholasticus, a rather prosy, old-fashioned gentleman whose evident aversion for the times in which he lives and for the manners of those times forbids our taking him too seriously.

The symposium method may, I think, like any other method under the sun in the wide field of thought-husbandry, be made to stimulate and protect the growth of sound arguments as well as of fallacies. Of what kind it has brought forth on this occasion I must leave it to my patient hearers to determine.

It is both trite and untrue to assert that the south is backward in methods, but rather in lack of money to execute and realize methods. By careful inspection of the curricula of a list of southern institutions, even the smaller fresh-water colleges, we shall find that mention is nearly always made of the laboratory and of the library. Besides, a majority of these colleges are manned by competent instructors—young men educated after the most approved orthodox modern methods in American and European universities. This is especially true in the departments of the natural sciences, and particularly in chemistry. The crux of the situation is the poverty of all of our colleges, including state institutions, private foundations, and church colleges alike.

Of the three requisites for a good college, competent teachers, eager students and adequate equipment, naming them in the order of their importance—I take it that

[1] Paper read at the meeting of the American Chemical Society at New Orleans, December 30, 1905.

southern colleges possess the first two in a liberal degree. In our poverty, however, we have been consuming time in applying the first law of nature, that of self-preservation; and have often chosen to do those things which we should have preferred to alter or forego.

With a desire to contribute towards bringing about a better understanding and feeling among the teachers of chemistry in the south, I recently sent out a number of inquiries, the answers to which should form a symposium on chemistry requirements. The results have been most gratifying. Of the forty-four teachers of chemistry concerned, forty made prompt, candid and interested replies.

The list of questions by no means covers the ground, but enables us to form some idea of the general trend of work done in the more important southern colleges.

In order that the replies may be discussed systematically, I have here arranged the several queries *seriatim*, as in the circular, with their corresponding replies.

1. *Should chemistry be taught in preparatory schools?*

Of the forty replies, twenty are affirmative, fifteen negative and five doubtful. My object in asking the question was to secure some justification of the decision of the Committee on Chemistry Entrance Examinations for the Association of Colleges and Preparatory Schools of the Southern States. The committee is made up of Dr. Cameron Piggott, Sewanee; Dr. F. P. Venable, University of North Carolina, and myself. The majority of the committee were opposed to the study of chemistry in preparatory schools, and asked to be excused from service on that account. But at the earnest solicitation of the president of the association, the committee remained intact and submitted a list of college entrance questions. From the replies to question 1 of the circular, it seems that the majority of southern chemistry teachers differ with the committee, and approve of the study of chemistry in preparatory schools.

2. *Does your first course in chemistry precede, accompany, or follow that in physics?*

The replies show that in ten colleges chemistry precedes physics; that in eight it accompanies physics; and that in twenty-two, it follows physics. These figures, however, are probably not expressive of the ideals of the respondents, but form rather a simple statement of facts and conditions as they find them. In many cases neither subject is required for entrance and both are postponed until after college is entered. In nearly all cases chemistry is begun in the college. In a large number of cases physics is begun in preparatory schools.

I believe, personally, that physics should invariably precede chemistry. Elementary physical phenomena are more easily observed and interpreted than those of chemistry; and, besides, while very few physical problems involve chemistry, nearly all chemical problems require some physics for their solution.

Another consideration to be reckoned with is that a beginners' course in physics may be conducted more successfully and satisfactorily with limited equipment than a course in chemistry. In fact, I am rather strongly persuaded that the study of chemistry should never be undertaken in any of our southern preparatory schools. The time spent in such schools should be devoted to the humanities and the mental disciplinary studies, such as literature, language, pure mathematics, and sufficient nature study to give diversity and recreation. When students begin chemistry, their minds should be able to grasp such

abstractions as the atomic theory, Avogadro's hypothesis and so forth.

3. (*a*) *State your ratio of hours devoted to lectures and laboratory in general chemistry.*

(*b*) *Should individual laboratory work invariably be required in connection with the study of general chemistry?*

The replies to (*a*) show general uniformity in ideal and application. The theory seems to prevail that about equal time limits be afforded lectures and laboratory. In most cases the practise conforms to the theory. There are some extremes which, when paired, do not affect the average. One college gives the ratio three lectures to nine laboratories; another reports five lectures to one laboratory.

I was glad the answers to (*b*) were made *in absentia*, for a majority were very emphatic in their affirmative replies—so much so that italics and exclamation points were in evidence. I should have been embarrassed had I asked the question of an audience of chemists, and had their replies been as vociferous as I am led to suspect they would have been. The interesting feature of the symposium is that some answered directly in the negative, and other few in a modified affirmative. Dr. Caldwell, of Tulane, writes: 'Some laboratory work is advisable; but laboratory work is, in my opinion, often overdone'; Dr. White, of the University of Georgia: 'Not invariably'; Dr. Wait, of the University of Tennessee: 'We do not require laboratory work.'

I am afraid we have made a fetish of laboratory work. The pendulum has swung too far. Twenty-five years ago some good chemistry was taught by masters to students who did the minimum of laboratory work. At that time Harvard, Yale, Princeton, Virginia and other similar and smaller colleges gave courses in general chemistry with practically no laboratory work. It might be said that those times of laboratory ignorance could be winked at, but that now all chemists everywhere should be expected and urged to require laboratory work.

In the evolution of chemistry teaching we are facing the same problem which, in language teaching, provoked a wordy wrangle among the German universities fifty years ago. Leipzig placed grammar before literature; Berlin placed literature before language.

We must not lose sight of the fact that chemistry, while an experimental science, possesses likewise a large deductive value. Undoubtedly, we should require laboratory work in general chemistry; but if I were forced to accept an alternative, illustrated lectures without laboratory, or laboratory without lectures, I would choose the former, the lesser of the two evils.

4. *Does your course in qualitative analysis accompany or follow that in elementary general chemistry?*

For the most part the consensus of custom everywhere, including the south, is to let qualitative analysis follow general chemistry. I presume that in the case of this question I am also open to criticism from those who consider no other place for qualitative analysis. The object in asking the question was twofold: First, to emphasize the fact that many text-books include impossible and out-of-place courses. Just why qualitative analysis should be mixed with general inorganic chemistry I can not understand. The short references to the subject are nothing more than an outline, and I have never known a teacher to use them in actual practise.

The second purpose was to make a plea for legitimate qualitative analysis. The subject has been abused, it is true, and often authors and teachers have degraded

the science of chemistry to the art of detecting and separating acids and bases. In their over-ardor for brevity and tabulated schemes, they have emphasized the empirical and minimized the rational aspect of the subject, to its detriment as a factor in liberal education. The subject has a peculiar virtue if taught with the end in view that it is not technical but pure chemistry. By devoting periodical hours to the interpretation of the chemistry involved, no branch of the science offers such delightful and profitable employment to students.

I am aware that my view may possibly be regarded as old-fashioned, and that there is an effort to displace this subject from the college curriculum. Dr. Arthur Lachman, in a recent address before the American Chemical Society, referring to qualitative analysis, said: "The chemists of the U. S. Geological Survey never carry out qualitative analyses of the rocks they investigate. * * * The assayer never makes other than a quantitative analysis of gold and silver ores. For the food analyst, all is grist that comes to his mill—moisture, fats, carbohydrates, proteids and ash. Where then is our boasted art of qualitative analysis?" Evidently the speaker has misinterpreted or forgotten the aim and genius of qualitative analysis. As an art it is useful only as a handmaid to the science. It is not technical chemistry, and I doubt whether Liebig or Fresenius ever considered it in that light. The mineral analyst does not rethresh old straw by investigating the constitution of the positive and negative ions he discovers. This he learned in general chemistry. Nor does he waste his time in making a qualitative analysis of the average specimen; for experience has taught chemists that most minerals are composed of only a few basic and acid radicals. The argument for the condemnation of qualitative analysis would apply with equal force to general chemistry.

The object of qualitative analysis is not so much the detection of certain ions in 'unknowns,' but rather a systematic study of the chemistry involved in a well selected and progressive set of facts. I use the word 'progressive' advisedly, for though the arrangement is not that prescribed by Mendelejeff, it is the most practicable known. Nor do we follow the periodic table in the study of general chemistry, for the apparent reason that oxygen, hydrogen, nitrogen and chlorine are more accessible and tractable than either lithium at the one end, or fluorine at the other end of the table.

5. *What number of hours do you devote to qualitative analysis?*

The average time calculated from these replies is six hours a week for one term or semester. I am glad to learn that there seems to be no desire nor expectation of limiting or abandoning qualitative analysis.

6. *What number of hours do you devote to quantitative analysis of typical compounds before attempting technical analysis?*

The language here is somewhat obscure, but as yet I am unable to formulate a sentence more clearly expressing my thought. Hardly any two authors or teachers would give identical lists of typical compounds for courses in quantitative analysis; though probably all would include in their lists a dozen or more common types.

It is not surprising that a great diversity of opinion and practise is shown in these replies. After all, the *desideratum* is a thorough drill in gravimetric and volumetric methods. Familiarity and practise with the balance and other essential apparatus, including calibration of graduates and preparation of reagents, are funda-

mental, but are attained by various individual methods.

7. (*a*) *Do you begin the study of organic chemistry with a lecture course merely, or with one combining lectures and laboratory work?*

(*b*) *What number of hours do you devote to organic chemistry?*

I happened to know of several southern colleges which give lecture courses in organic chemistry without laboratory work. I was anxious to learn how general this practise is in the south; and accordingly used this opportunity of taking a census of southern colleges on this subject.

Those which give no parallel laboratory work, though in the minority, are more than I had supposed. Just what the trend in this method of teaching organic chemistry is I have not been able to learn. Whether the large number of lecture courses without laboratory is a protest against what some regard as a laboratory mania, or whether the majority of teachers following this practise have not adequate laboratory equipment or assistance, I am not prepared to say. I suspect, however, that the latter is true, though the list includes several small colleges and a smaller number of universities and well-equipped schools of technology. The following are some of the institutions which offer organic chemistry lectures without laboratory: Tulane, Sewanee, Washington and Lee, University of Louisiana, University of Tennessee, University of North Carolina, Clemson College, A. and M. College of Texas.

What was said regarding general chemistry without laboratory may also, I think, be said in part of organic chemistry without laboratory. It is better in most cases to have the laboratory course, but often the deductive may precede the inductive with fair results. Another factor to be taken into consideration is the stage of the student's chemical education when the subject is begun. If introduced early, before he has acquired the quantitative habit, I think that laboratory work should invariably accompany lectures, the idea being to reinforce theory with practise. If the student has had considerable training in both theoretical and practical inorganic chemistry, it seems reasonable to believe that he could assimilate lectures on organic chemistry without the laboratory.

The average number of hours devoted to organic chemistry is three hours for lectures and two hours for laboratory a week for one term.

8. (*a*) *Do you offer courses in chemistry for graduate degrees?*

(*b*) *If so, please state the degrees.*

The majority of the institutions represented offer graduate work in chemistry for the M.S. degree. This is noticeably characteristic of the southern state universities. The most significant, and I think hopeful, development from the investigation is the very small number of southern universities offering the Ph.D. degree. Of the forty institutions heard from, only the following offer the doctor's degree: University of Virginia, Washington and Lee, University of North Carolina, Vanderbilt, University of Mississippi, Tulane, University of Missouri, Washington University. Of this number, four, while still offering the degree, indicate their intention of abandoning it. The small number is hopeful, I think, in that it means a growing desire among our southern colleges and universities to accomplish only that work they are best fitted to do. Many have been pretending to do work for which they were not adequately equipped. The cause is less one of vanity or dishonesty than of history. Before the civil war, nearly every southern state had well-equipped

state and church colleges. Many of them were legitimately named universities and did what was then regarded as university work. The war destroyed their endowments and buildings, and left most of them with only one asset, their names. When they were reopened, after the war, it was hard for some of the universities immediately to become colleges, and for the colleges to become preparatory schools. In many instances their ante-bellum titles carried with them grants, franchises and endowments, rendering it practically impossible for them to assume more modest and significant names. I am quite sure that the authorities of a majority of the so-called universities of the south would prefer that their institutions be called colleges; but for the reasons assigned and others, they have been largely powerless to change the titles. Furthermore, some institutions, which enjoyed merited reputations before the war have tried to justify their pretensions by indulging in the hope that they may come into their own again.

But whether we praise, apologize for or blame our institutions, the south is in this matter justly criticized, though pretence is rapidly disappearing, and most southern colleges are honestly trying to live up to their published standards. Some, however, still continue to publish courses which either are not taught, or are conducted in a perfunctory manner. I shall not call any names, but I am venturing the statement that ten of the forty institutions on the list publish courses in their catalogues different from those given in the circular now under discussion.

I have already digressed, but must ask leave to wander a little farther yet.

There are too many colleges in the south. It is a mistake to substitute quantity for quality. It were better for the south to have fewer, well-equipped colleges than so many with inadequate appointments. It is too late and impracticable now for each state to focus all of its higher state institutions into one common plant. The present generation is not responsible for the mistakes of the past, and can not correct some of them; but it is responsible for the future. There should be no more dissipation of our educational forces, and all future enlargement should be added directly to the central university. Probably no state in the south makes more liberal appropriations for higher education than Georgia. Two sets of schools and colleges for the two races must necessarily be supported. We believe that this is fundamental and should be maintained at all hazards. But is it educationally economical for Georgia to provide seven separate, unarticulated institutions, so situated that students from no one of them can have access to the instruction and equipment of the others? Other states, especially those in the west, are consolidating their public institutions into great university systems.

9. *What are your requirements in inorganic preparations?*

10. *What are your requirements in organic preparations?*

Eight of the list of colleges require both organic and inorganic preparations additional to their elementary courses in these subjects; three of them, organic preparations only; and two, inorganic preparations only. One of the largest universities in the south, Tulane, has no formulated prescription for either organic or inorganic preparations. Dr. Dudley, of Vanderbilt, writes, 'I never could see the necessity for inorganic preparations.'

Why so few southern chemistry teachers carry on research themselves, much less offer courses, is patent, when we consider their situation. They are so overloaded

with instruction or executive duties that they can not carry on original work in their own specialties. Nor are they supplied with adequate library or laboratory facilities for advanced students.

11. (*a*) *Do you offer technical courses in chemistry?*

(*b*) *If so, which do you emphasize?*

There are more southern colleges offering technical work than I had anticipated. Twenty-four of the list give technical courses in some form or other. Usually the special kind of technical work is controlled by local demands and natural supplies. For example, all of the colleges receiving the Morrill fund make a specialty of fertilizer, soil and food control; the Louisiana colleges offer courses in sugar chemistry; the Alabama colleges, iron and coal analysis; and so forth.

I consider this both pedagogical and profitable. While the study of chemistry should be considered fundamental, and should not be side-tracked for technical work too soon in a student's chemical education, even from an educational viewpoint some technical application is helpful in reinforcing previous theoretical training. The same principle obtains in language study. Literature emphasizes and strengthens the technique of grammar and rhetoric. Then, too, technical training is materially profitable. The reason why the familiar national trade-mark, 'Made in Germany,' is a valuable asset is because German manufactories are worked and superintended by graduates technically trained in the great universities. It is possible that the *Germanophobia* indulged in by our British cousins is caused by commercial rather than political jealousy. The two antipodal economic practises account for the apparent industrial decadence of Great Britain compared with the ascendency of Germany. Oxford and Cambridge—despite their admirable history of light and leading—have had little share in the affairs of the great industrial centers of Birmingham, Manchester and Sheffield. These industries have been developed by the apprentice system. Under this system young men become skilled in the manipulation of old methods, but have no opportunity of contact with the new. In one German pharmaceutical plant alone, there are employed two hundred university graduates, who by their superior skill and education not only perfect old methods of preparations, but are earnestly devising new processes. Although Great Britain and America manufacture more iron, and consume more coal, Germany, a less favored nation in these raw materials, converts the by-products from our coke ovens and gas plants into antipyretics and coal-tar colors, and returns the finished products to us.

President Remsen in his inaugural address at Johns Hopkins said: "It is generally accepted that the reason why Germany occupies such a high position in certain branches of industry, especially those founded upon chemistry, is that the universities of Germany have fostered the work of investigation more than those of any other country. * * * In Germany the chemical industries have grown to immense, almost inconceivable, proportions. Meanwhile, the corresponding industries of Great Britain have steadily declined."

Once the American universities were replicas of the British system, but now the German university sets the standard.

It is this shifting of method and manner that affords us of to-day, in the matter of the practical virtue of our courses in science, an assured guarantee of commercial and industrial progress.

J. F. Sellers.

Mercer University.

SCIENTIFIC BOOKS.

Status of the Mesozoic Floras of the United States. By LESTER F. WARD. Second Paper, with the collaboration of WILLIAM FONTAINE, ARTHUR BIBBINS and G. R. WIELAND. U. S. Geological Survey, Monograph XLVIII., 1905. Part I., text, pp. 616; Part II., plates I.–CXIX. 4to.

This valuable contribution to the paleontology of North America, and particularly of the United States, appears as a sequel to an earlier publication on the Older Mesozoic,[1] and forms the second in a series of which the third is yet to appear. Like the previous publication, the present one deals with the floras of widely separated localities and with a somewhat similar range of horizons. It is divided into three parts: Part I. deals with the Triassic Flora as presented by the Older Mesozoic of Arizona; Part II. discusses the Jurassic Flora of Oregon, Wyoming and the Black Hills, as well as the transition floras of Alaska, California, Montana and Oregon; Part III., which occupies the largest share of the volume, is devoted to the Cretaceous Flora as presented in the Queen Charlotte Islands, the Kootanie of Montana and British Columbia, the Lakota (Kootanie) of the Black Hills, the Trinity of Texas and the Older Potomac of Maryland and Virginia. The flora of the Black Hills having been dealt with somewhat fully in a previous paper,[2] the present account is somewhat brief, discussing supplementary data which are, nevertheless, of considerable importance. If any estimate of comparative value were to be made, it would be in favor of Part III., because of the important results reached with respect to evidence bearing upon the correlation of the Potomac in various localities and its precise relations to the Jurasso-Cretaceous.

The opening chapter deals somewhat briefly with the Triassic, or the Older Mesozoic flora of Arizona, based upon data secured by an expedition executed in May and June of 1901. While the results obtained were especially rich in vertebrate paleontology, some important observations were made with respect to the occurrence of plant remains which were, nevertheless, found to be exceedingly scanty. This appears to be accounted for by the great abundance of gypsum that prevails throughout all the beds of the region, a material which is fatal to the preservation of more delicate plant remains, and in consequence it is only the silicified wood that seems to have escaped its influence. Furthermore, such silicified woods are mainly deposited in beds of sand, coarse gravel or conglomerate, which in themselves are unfavorable to the preservation of plant impressions. As the petrified forests have been reported upon at length on a former occasion,[3] and as the structure of the woods has been worked out by Knowlton,[4] at least in part, they do not claim special attention. Nevertheless, there are frequent references to the abundant occurrence of fossil woods which would presumably afford an opportunity for an important extension of our knowledge respecting extinct species; but the impression conveyed to the reader is that the investigator gave somewhat scant heed to this most important material, attaching far more value to mere leaf impressions, fruit and other forms of remains. The impression thus gained is heightened by the want of any sort of reference to the working up of the wood, except such as may be found in the statement that 'The only species that has yet been described from the silicified wood of Arizona is *Araucarioxylon arizonicum* of Knowlton, based on specimens from these two trunks."[5]

[1] Ward, Lester F., 'Status of the Mesozoic Floras of the United States.' First Paper: The Older Mesozoic. U. S. Geol. Surv., XX., Part II., 1898–1899, pp. 215–430, pl. XXI.–CLXXII.

[2] Ward, Lester F., 'The Cretaceous Formation of the Black Hills as Indicated by the Fossil Plants.' U. S. Geol. Surv., XIX., 1897–1898, pp. 527–712, pl. LVII.–CLXXII.

[3] Ward, Lester F., 'Status of the Mesozoic Floras of the United States.' U. S. Geol. Surv., XX., 1898–1899, pp. 316–332.

[4] Knowlton, F. H., 'New Species of Fossil Wood (*Araucarioxylon arizonicum*) from Arizona and New Mexico,' *Proc. U. S. Nat. Mus.*, XI., 1888, pp. 1–4.

[5] This reference is to two great logs taken to the National Museum in 1880 or 1881, from Lithodendron Creek, by an expedition headed by Lieut. J. T. C. Hegewald in the spring of 1879.

The conditions prevailing in the petrified forest are interesting and deserving of somewhat special notice. The deposits of the Grand Cañon region, usually spoken of as a single great group of beds, are reduced by Ward to three entirely distinct formations. The lowest, presenting their full development at the mouth of the Mœncopie Wash, are known as the Mœncopie beds; the second and thickest member of the series is termed the Shinarump—a name already employed in this connection by Major Powell; while the third and highest member is designated the Painted Desert beds.

The Mœncopie beds are almost entirely barren of animal and plant remains; for apart from reported fern impressions, the occurrence of which could not be verified, the only evidence of plant remains were to be found in a few impressions from the very base of the formation, and possibly not Mœncopie at all, but of an earlier horizon. These presented little evidence of structure, though they seemed to represent the characteristics of coniferous twigs and short branches, possibly of an Araucarian type.

The Shinarump formation within which the remains of trees are chiefly found, is separable into a lower or Lithodendron member, and an upper or Lereux member—the former embracing conglomerates, sandstones, clay and argillaceous shales forming brilliantly colored banded cliffs, while the latter consists at its base of a somewhat similar formation upon which are superimposed beds of sandstone, limestone, mortar beds with flints and calcareous marls. Apart from silicified tree trunks, the only observable plant remains consist of raised casts of twigs lying in all positions, and stems with whorled branches, all of which show Araucarian structure. These forms occur on the faces of sandstone rocks and shales, and they are provisionally designated as *Araucarites shinarumpensis*. The figures given of this material do not lead one to hope for any very definite knowledge of the plants they represent. The logs derived from the silicified trunks of trees do not, in all probability, represent the original distribution of the forest, since, as shown in the original report (*op. cit.*), there is evidence that they were drifted some distance before being laid down; and in spite of previous statements to the contrary, there is little evidence to show that any of them are erect and *in situ* except in the variegated marls just above the conglomerates, where a very careful examination led to the conclusion that a group of twenty or more stumps were actually in the places of original growth, although in a sedimentary bed. Many of the smaller buttes appear to have been developed by reason of the logs which weighted down the underlying marls and tended to prevent the latter from being blown or washed away, with the result that such logs may now be seen lying on the tops of the buttes, down the slopes of which the disintegrating material is continually rolling.

One remarkable specimen which requires further explanation, is met with in certain remains designated as *Araucarites monilifer*, in allusion to the peculiar necklace-shaped rows of resin drops which occur in the interior of the trunks, where they may often be seen in considerable numbers. According to the facts obtained, these bodies which have a stratiated surface, are resinous exudations into the interior of the stem, much as resin blisters are formed in the outer bark of the common balsam fir.

In the Painted Desert formation, the only remains of trees appear to be represented by tree trunks, the original structure of which has been wholly replaced by black sand, so that they resemble deposits of manganese or limonite.

The Jurassic Flora of Douglas County, Oregon, by Professor Fontaine, occupies some 99 pages of descriptive text and deals with no less than 77 species and varieties, of which fully 31 per cent. are new—the new species lying chiefly among the ferns and cycads. Among the conclusions to which a study of these plants points, we may note as of particular interest that there is a remarkably large proportion which are also common to the distant and widely separated regions of Yorkshire, England, and eastern Siberia. Professor Ward shows that there are fourteen species common to the Jurassic of Yorkshire,

and sixteen also common to the Jurassic of Siberia, a difference which will hardly justify conclusions based on numerical grounds alone, as to the probable connection between such regions. Professor Ward nevertheless infers from such evidence, that a definite land connection existed in Jurassic time, between Asia and northwestern America, and this opinion is strengthened by reference to the remarkable similarity with respect to number and kinds, of the types of *Ginkgo* found both in Siberia and Oregon. In this connection, however, it is instructive to recall the conclusions of Asa Gray with respect to similar relations as exhibited by the existing floras of the three regions mentioned.

From a study of Japanese plants collected by the Perry Expedition in 1852–1854, supplemented by subsequent collections, he found that out of a total of 580 Japanese plants, about 120 also occurred in western North America; 134 in eastern North America, and 157 in Europe. A further analysis of these relations showed that 'there has been a peculiar intermingling of the eastern American and eastern Asian floras, which demands explanation'; but upon a critical examination of the positions occupied by representative species, there is found abundant evidence in support of the view also held by Bentham, that the 'interchange between the temperate floras even of the western part of the old world and of the new has mainly taken place via Asia.' Gray further observes that such considerations 'suggest an ancient continuity of territory between America and Asia, under a latitude, or at any rate with a climate, more meridional than would be effected by a junction through the chains of the Aleutian and the Kurile Islands.'[6] Two independent chains of evidence are thus found to contribute to the same conclusion, while it is further clear from the paleobotanical evidence, that the continental connection thus indicated must have existed until at least the close of the Jurassic, and possibly later. As further justifying the position held by Professor Ward and also showing that the continental connection was not disturbed until after the close of the Jurassic, attention may be directed to the fact 'that the Oregon strata rival those of eastern Siberia in the development of ginkgos; while nearly all the more important species made by Heer from the Siberian beds have similar forms in the Oregon strata,' forms which also have been found to extend into the Lower Cretaceous of the United States and British Columbia.

One noteworthy feature of the Jurassic Flora as now presented is the addition of some 600 specimens of cycads from Wyoming, to the list previously reported by Ward[7] and then embracing descriptions of all the Jurassic Cycads from Wyoming known at that time.

In discussing a number of plants from Alaska, which show very strong evidence in favor of Lower Cretaceous age, but which are held to represent the Jurassic because of the strong representation of Jurassic types of *Ginkgo,* reference is made to some of the work of the late Sir William Dawson, with respect to which exception is taken to the identification of *Ginkgo lepida* Heer, a species which Ward holds must be a *Baiera.* Some doubt is also thrown upon the correct identification of *Ginkgo sibirica* Heer, both of these species having been recognized by Dawson in the Kootanie of Canada. Attention is drawn to this opinion at the present time because, as shown by recent studies of the Kootanie flora, Dawson's determinations were correctly made; and although, as stated by Ward, the ginkgos had become nearly extinct on the American continent in Lower Cretaceous times, even in those parts where they had been so prominent in the Lower Oolite, there is not wanting abundant evidence to show that, even though the only ginkgos of the Lower Cretaceous of the northwestern region are to be found solely in the Kootanie of Canada, this formation bears a representation of the genus which shows a very important extension from the Jurassic, abundantly confirming the earlier conclusions of Sir William Dawson. Recent collections from the eastern slope of the Rocky Mountains as well as from the Crow's Nest Pass, show an extension of the Kootanie flora

[6] 'Scientific Papers,' II., p. 135 et seq.

[7] U. S. Geol. Surv., XX., 1900, Part II., pp. 382–417.

not heretofore known. Among the well-defined species from these areas are a large number of ginkgos represented by leaves, male inflorescences and also by the fruit. The leaves, which furnish the most reliable data, show *Ginkgo sibirica* to be not only well characterized but abundant, while *G. lepida* is also present. A much less abundant, but none the less distinguishable type, is *G. huttoni* (Sternb.) Heer. We can not, therefore, longer regard *G. sibirica* as the sole survivor of this genus in the Lower Cretaceous. It must nevertheless be conceded that the occurrence of these three species in the Kootanie of British Columbia and Alberta not only emphasizes the basal character of that formation, but it directs attention to the probability that its lowest portion may even represent a transition series—a conclusion which would bring it into agreement with the views recently expressed with respect to its equivalent in the United States, the Lower Potomac.

Our knowledge of the Shasta-Chico series of California, originally defined in detail by Diller and Stanton,[8] receives a fresh interest by virtue of the treatment of the Shasta group at the hands of both Professor Ward and Professor Fontaine. The more recent recognition of this series in Oregon is of interest in connection with the lately made observations that it is also to be met with in British Columbia, where a newly described element of the flora of this series as developed in California (*Gleichenia gilbert-thompsoni* Font.) has been recognized.

In discussing the Kootanie formation, the valuable work of Sir William Dawson and Dr. George Dawson is fully recognized, and correlations with the United States horizons are established. The value of this discussion is greatly enhanced by the introduction of a list of all the Kootanie plants recorded up to 1895. More recent studies of this formation have extended this list in important particulars, while they have also brought out the fact that the Kootanie flora of British Columbia and Alberta is more widely distributed on the eastern slope of the Rocky Mountains than has been hitherto recognized.

[8] *Bull. Geol. Soc. Amer.*, 1894, pp. 435–460.

Somewhat less than half the entire volume (257 out of 599 pages) is devoted to a discussion of the Older Potomac Flora, a term which is here employed in the same sense as in his previous paper on the Potomac Formation (Fifteenth Ann. Rept. of the U. S. Geological Survey), representing in the main the Potomac as it occurs in Virginia, and including all beds of the same age occurring in other states, but excluding those higher beds in which the flora is mainly dicotyledonous. A very large proportion of this part of the subject is devoted to an interesting and valuable historical survey of our knowledge of the Older Potomac Formation.

In deriving conclusions from his examination of the flora, Professor Fontaine endeavors to establish a correlation between the Potomac formations of Maryland and Virginia, and in doing so he points out that the term Lower Potomac, as consistently used by him, embraces the four members recognized by Ward as (1) James River, (2) Rappahannock, (3) Mount Vernon and (4) Aquia Creek—the second being identical with what he had designated as Fredericksburg, and the last or fourth being the same as his Brooke beds. On the other hand, the Maryland Geological Survey has divided the entire formation into (1) Patuxent, (2) Arundel, (3) Patapsco and (4) Raritan in ascending order, but in neither of these is there a question of division into Lower and Upper Potomac. Under these circumstances, Professor Fontaine finds it necessary to explain what is meant by Lower Potomac. The Potomac of Maryland differs from that of Virginia in the absence of the Mt. Vernon member, but the chief difference which appears to distinguish the formation of the two states seems to lie in the absence of the Raritan from Virginia, while it is in force in Maryland, and if the Raritan is to be regarded as Potomac, about which Professor Fontaine appears to have some doubt, then it constitutes the Upper Potomac, while all below is Lower Potomac. This conclusion is apparently based upon the observation that there is essentially only one great break in the continuity of the flora, and that occurs in passing from the underlying beds to the Am-

boy Clays. There are, of course, well-defined changes between the lower beds of the Potomac, but such changes are gradual, due to the diminution of old types, accompanied by the increase and introduction of more modern forms, while the flora as a whole presents essential continuity. But in passing to the Amboy Clays the case is wholly different, and a wholesale change occurs whereby few of the older types survive. At the same time a great number of new plants appear, and dicotyledons overwhelmingly predominate. In view of this very striking change in the character of the flora, Professor Fontaine puts the very pertinent question, 'Why give the name Potomac to this Group?'

With respect to the much-debated question of the precise relation which the Lower Potomac bears to the Jurassic on the one hand, and to the Cretaceous on the other, there can be no doubt as to the transitional character of the flora as presented by many of its components, but the real 'question as to the Jurassic or Lower Cretaceous age of the Lower Potomac hinges upon the position of the Wealden formation.' Previous studies of the Lower Potomac plants by Professor Fontaine[9] had led him to express the opinion that they indicated a Lower Cretaceous age agreeing with the Neocomian. This conclusion was based on the strong affinity of its flora with that of the Wealden, it being assumed that the view generally held as to the position of the Wealden is correct, that is, that it is the non-marine equivalent of the Neocomian. In spite of the view held by Professor Marsh, Professor Fontaine finds that there has been no evidence sufficient to cause a change of his former expression of opinion, but, on the contrary, a good many facts have come to light that confirm its correctness.

These studies of the Potomac flora indicate that the Potomac formation had a widespread development on this continent, since it is not only recognized in Virginia and Maryland, but to the south and west it extends to Tlaxiaco in Mexico; while on the north and west it reappears in the Shasta of California, the Lower Cretaceous of Queen Charlotte Islands and in the Kootanie of Montana, British Columbia and Alberta.

[9] U. S. Geol. Surv., Monograph XV., 1889, p. 348.

D. P. PENHALLOW.

MONTREAL,
April 27, 1906.

A Respiration Calorimeter with Appliances for the Direct Determination of Oxygen. By W. O. ATWATER and F. G. BENEDICT, of Wesleyan University. Washington, D. C., The Carnegie Institution of Washington. 1905. 4to, pp. 193, 49 figs.

The apparatus for investigations in human nutrition described in this monograph has been in process of development for about twelve years and in its perfected form may be safely characterized as the most elaborate instrument for physiological research at present in existence. Descriptions of the apparatus in its earlier form, and of the various improvements and modifications introduced from time to time, as well as accounts of investigations carried on with it, have been published by Professors Atwater, Woods, Rosa and Benedict as bulletins of the Office of Experiment Stations of the U. S. Department of Agriculture and also as a memoir of the National Academy of Sciences.

The apparatus as described in the publications just mentioned consisted of a Pettenkofer, or 'open circuit,' respiration apparatus, the chamber of which was so constructed as to serve likewise as a calorimeter. With this instrument very accurate determinations were possible of the income and outgo of carbon, hydrogen, nitrogen, mineral matter and energy in man, but the results were incomplete, inasmuch as no direct determination of the amount of oxygen consumed by the subject could be made. With the aid of a grant from the Carnegie Institution, therefore, the authors undertook a reconstruction of the apparatus with this object in view.

For this purpose they have reverted to the earlier type of respiration apparatus, originated by Regnault & Reiset and often designated as the 'closed circuit' type, in which the air after leaving the chamber of the apparatus is freed from respiratory products, re-

plenished with oxygen and returned to the chamber. This type of respiration apparatus has always been recognized as being the most perfect in theory, but serious practical difficulties have been found in its use, and the more easily manipulated if less perfect Pettenkofer apparatus has been the type most commonly employed. It was a bold attempt, therefore, to reconstruct a complicated apparatus, in a scale sufficient for experiments on man, and the authors are to be congratulated upon their success in devising the first practicable large apparatus of this type.

It is, of course, entirely outside the scope of a review to enter upon even an outline description of the apparatus. A study of the monograph can not fail to impress the reader with two things—the ingenuity displayed in the devising of the various parts of the apparatus and the unusual amount of care which has been devoted to the search for sources of error and the determination of their probable magnitude. In the latter respect the volume affords an instructive example of true scientific accuracy, consisting not in inerrancy, but in a critical estimate of the degree of approximation to the truth. Noteworthy, too, is the very interesting method of computing the results of the respiration experiments by which they are made to a large degree to check each other. Check tests of the accuracy of the apparatus as a calorimeter have been made, in which known amounts of heat were generated in it electrically, and also so-called alcohol check tests, in which known quantities of ethyl alcohol were burned in the apparatus and the evolution of carbon dioxide, water and heat and the consumption of oxygen were compared with the theoretical amounts. The observed results differed from the theory by less than one per cent., thus justifying the claim of the authors that the results approach in accuracy those of the most approved methods of chemical analysis.

The monograph closes with a description of one of the numerous experiments on men which have been made with the apparatus, the experimental periods covering from one to thirteen days, and which have demonstrated its entire practicability. American science is to be congratulated upon the addition to its resources of this exceedingly valuable instrument of research, and the Carnegie Institution has performed a great service in rendering its construction possible.

H. P. Armsby.

Studies in General Physiology. By Jacques Loeb. The Decennial Publications, Second Series, Volume XV. Chicago, The University of Chicago Press. 1905.

These studies present a collection of widely scattered papers of Loeb on subjects of general physiology. The two volumes contain 37 papers, monographs, essays and shorter papers, only 13 of which were previously published in English. The publications cover a period of fourteen years, from 1889 to 1902. Some of these papers were published in pamphlet form and were quite inaccessible. These papers present by no means all the studies which this productive investigator has published during that period; nor are there included in this collection such studies which were published in conjunction with some of his associates and pupils—a fact which the reviewer can only regret. With the exception of two or three short papers, every study in this collection presents a more or less extensive original investigation on some biological subject, invariably bringing to light new facts and new points of view. Although these studies deal with a great variety of diverse subjects, there is one apparent background to them all: it is the aim to discover the physical and chemical causes of living phenomena.

The papers are arranged chronologically: the first of them dates from 1889 and the last was published in 1902. The great variety of subjects treated in these numerous papers might, perhaps, be classified in the following four groups: 'Tropism,' 'Physiological Morphology,' 'The Physiological Effects of Ions' and 'Artificial Parthenogenesis,' intermingled with a few miscellaneous subjects not exactly belonging to any one of these groups. The chronology of these papers helps us to get an insight into the gradual development of the

diverse problems in which our author has done pioneer work. The following incident might give us perhaps the key to his starting point. From 1886 to 1888 Loeb was assistant in (animal) physiology in Würzburg. At that time the chair of botany at that university was occupied by Julius von Sachs, one of the foremost plant physiologists, who made a special study of tropism in plants and whose celebrated lectures on plant physiology appeared in 1887 in a second edition. This great investigator apparently exerted a lasting influence upon the direction of Loeb's searching mind. Thus we find that the first larger piece of work of our author consists in a pamphlet entitled: 'The Heliotropism of Animals and its Identity with the Heliotropism of Plants.' That pamphlet forms the first paper of this collection. Through a number of ingenious but simple experiments it is shown for the first time how the dependence of animal movements on light is in every point the same as the dependence of plant movements on the same source of stimulation. In the next paper it is shown that the same holds good also for the movements of sessile animals. In other papers which followed, the influence of gravity upon the movements of animals (geotropism) and the influence of contact irritability (stereotropism) were studied and were also found to be identical with the same influences in plants. The similarity of these phenomena in animals and plants demonstrated to Loeb their independence of a nervous mechanism, and in a paper on 'Instinct and Will' he comes to the conclusion that what has been taken for the effect of 'will' or 'instinct' is really the effect of light, gravity, friction, chemical forces, etc. In a study upon 'Heteromorphosis' he shows that by the above-mentioned physical influences, as in plants, the regeneration in some animals would lead to the production of an organ different in form and function from the original one. In this study the factor of turgescence, of hydrostatic pressure is mentioned for the first time. In a further study on 'Organization and Growth' upon marine animals it was found that besides the above mentioned physical factors, the concentration of the sea water was an important factor, there was no growth nor regeneration in concentrations above 5.4 per cent. nor below 1.3 per cent. Furthermore, the presence of oxygen as well as of potassium and magnesium was indispensable. From now on we meet with studies in which the importance of oxygen and especially of osmosis as physiological factors were considered in the first place. We meet them in the 'Experiments on Cleavage,' in the studies 'On a Simple Method of Producing from one Egg two or more Embryos which are Grown Together,' in the studies on the 'Sensibility of Fish Embryos to Lack of Oxygen and Loss of Water,' etc. Meanwhile the studies of Van't Hoff, of Arrhenius and of Ostwald upon osmotic pressure and dissociation of electrolytes created a new epoch in the sciences of physics and chemistry, and we find Loeb henceforth profoundly engaged in unraveling the mysteries of life with the aid of the newly established science of physical chemistry. The fruit of these new efforts we find laid down here in numerous papers on 'Artificial Parthenogenesis,' on the physiological effects of ions, on ion-proteids, on the effect of ions on contractility, on the toxic and antitoxic effects of ions, etc. It is, of course, impossible to give here any intelligible account of the multitude of important new facts laid down in these papers. We have here before us the fruit of a most indefatigable and ingenious investigator who has done pioneer work in many fields in biology. These studies will be a source of instruction and stimulation to many an earnest student in general physiology, and we ought to be thankful to the author as well as to the editors of the Decennial Publications of the University of Chicago for presenting to us the collection of these very valuable studies.

S. J. MELTZER.

ROCKEFELLER INSTITUTE.

SCIENTIFIC JOURNALS AND ARTICLES.

The American Naturalist for April contains but three papers: the first 'The Freshwater Copepoda of Massachusetts,' by A. S. Pearse, adds seventeen species, two new, to

the eight previously recorded. James B. Pollock discusses at some length 'Variations in the Pollen Grains of Picea excelsa,' and A. M. Reese describes the 'Anatomy of *Cryptobranchus alleghaniensis*.' Some comparisons are made between the skeleton of this species and that of the great salamander of Japan but no deductions are made as to their generic identity or differences. No reference is made to Cope's description of the anatomy of *Cryptobranchus*, though this is given at some length with one or two good figures in his Batrachia of North America.

The Museums Journal of Great Britain for March contains a good account of 'The New Zoological Institute at Breslau' by W. E. Hoyle. The idea of preservation of material in the exhibition series is carried to the extreme, for the museum is open to the public but twice a week, and then for only two hours. J. T. Wilson gives a description of the relations existing between the state and 'The Australian Museum, Sydney, New South Wales.' Professor Wilson concludes that in Australia the only practicable form of museum administration is by a board of directors, or trustees; that municipal control is quite out of the question, and that any autocratic form of administration by a director, in the case of an institution supported by public funds, would not be tolerated. In this respect then Australia would seem to differ from other parts of the world where the best form of museum administration seems to be by a responsible director. Mr. Edward Lovett presents in some detail a scheme for a folk museum and there is a brief account of the meeting of the Swedish Museums Association. Among the notes is one stating that in the Hamburg Museum the group illustrating a North Sea oyster bank has been enlarged and reinstalled and cases added showing the growth of precious coral and sponges. Hamburg is certainly to be congratulated on having such a progressive institution.

The Zoological Society Bulletin for March is styled the Aquarium Number and devoted entirely to the New York Aquarium or matters thereto pertaining. Among other articles it contains the following conundrums: 'Are Fishes Killed by Freezing?' 'Do Fishes Sleep?' 'Do Fishes Hear?' 'Have Fishes Memory?' The last three are answered in the affirmative, the first in the negative. And yet we have known every fish in a large but shallow pond to be killed during an unusually severe winter, though the possibility is that they died from lack of air. Fishes of the pike family may eat but little in an aquarium during winter, but in the New England states many more pickerel are taken through the ice than at any other season. There are interesting articles on the 'Remodeling of the Aquarium'; 'The Sounds Made by Fishes'; 'How Fishes Change Color'; 'Four Years' Change in the Axolotl,' besides many notes. The number is profusely and well illustrated. As a parting shot we should like to express our disbelief in green turtles or loggerheads exceeding 1,000 pounds in weight and would be glad to see one of half that size.

SOCIETIES AND ACADEMIES.

THE TORREY BOTANICAL CLUB.

The club met in the American Museum of Natural History, March 13, 1906, at 8 P.M. President Rusby was in the chair and thirteen persons were present.

A communication was read from Mr. Ellsworth Bethel, of Denver, Col., stating that he and Dr. Sturgis were at work on the fungi of Colorado, and would soon publish their first number, listing the Myxomycetes of the state.

President Rusby presented the matter of public recognition by the club of the coming tenth anniversary of the establishment of the New York Botanical Garden, and the appointment of Dr. Britton as professor emeritus of botany in Columbia, and of Professor Underwood as Torrey professor.

Motion was made and seconded that a committee be appointed by the chair to make arrangements for such an event. The motion was carried, and the president appointed the following committee: Miss Vail, Miss Marble, Dr. Murrill, Dr. Curtis, Professor Richards, Dr. MacDougal and Dr. Barnhart.

The first paper on the scientific program

was by Dr. P. A. Rydberg, entitled 'Botanizing in Utah.'

The substance of this paper appeared in the *Journal of the New York Botanical Garden*, 6: 158. 1905.

Many herbarium specimens were passed, illustrative of the paper.

The last paper was by Professor E. S. Burgess, on 'Biotian Asters.'

This paper was an informal discussion of the Biotian section of the genus *Aster*, accompanying the publication at this time of the author's monograph on the Biotian Asters (constituting Vol. 13 of the Torrey Club's Memoirs) with description and figures of 84 species and 10 subspecies, and with informal description of about 250 less definite forms. The Biotian section of *Aster* is one of the most difficult and variable, and seems particularly active in production of new forms, some other sections of *Aster* being quite stable in comparison. Most of the larger and more conspicuous species, with violet or lavender rays and glandular hair, constitute as a subsection the Macrophylli, typified by the well-known *Aster macrophyllus* L. Specimens illustrating the principal species of this subsection were exhibited and compared, and the speaker described the results of his method of continued observation on plant-colonies in unchanged natural habitat, with reference especially to the development of variations, and to distinction between certain changes apparently due to environmental conditions and other changes suggesting origin by mutation. Many herbarium sheets were shown illustrating the paper and especially the marked variations in this group of asters.

THE club met at the Museum Building of the New York Botanical Garden, March 28, 1906. In the absence of President Rusby, Dr. C. C. Curtis was called to the chair.

Dr. J. K. Small presented the first paper on the scientific program, on 'Additions to the Flora of Florida.' Specimens of the surface soil and subsoil, herbarium and alcoholic specimens, maps and photographs illustrated the paper.

The second paper was by Dr. J. H. Barnhart, on 'The Dating of Botanical Publications.'

Dates of issue of publication have always been of more or less interest to bibliographers, but modern biological nomenclature, with priority of publication as one of its fundamental principles, has emphasized to a marked degree the importance of determining accurately the exact time when novelties are placed before the scientific public.

The novice usually accepts without question the date printed on a title-page. Soon, however, he discovers a book with a clear, definite, unmistakable reference to one bearing a later date—perhaps a year or two, possibly many years. Here, then, he has evidence, amounting to convincing proof, that at least one of the books he has been consulting is incorrectly dated; but he may find it difficult to determine which is wrong, and still more difficult to replace the erroneous date by the correct one. Few, even of experienced botanists, realize what a large percentage of the literature of our science is labeled with misleading dates.

The purpose of the paper was to call attention to some of the causes of this state of affairs, to furnish examples of various classes of erroneous dating, and to mention certain precautions the observance of which will reduce the percentage of errors in the citation of dates. Many publications were shown to illustrate the paper. C. STUART GAGER,

Secretary.

THE MICHIGAN ORNITHOLOGICAL CLUB.

THE annual meeting of the Michigan Ornithological Club was held in conjunction with the annual meeting of the Section of Zoology of the Michigan Academy of Science in the Museum lecture room, Ann Arbor, on Friday, March 30, 1906.

The meeting was called to order by Chas. C. Adams, vice-president of the section of zoology. The following were the papers presented dealing with ornithology:

FRANK J. PHILLIPS: 'Bird Dissemination of *Juniperus*.'

MAX M. PEET: 'An Ecological Study of the Birds of Ypsilanti Bayou.'

Norman A. Wood: 'Twenty-five Years of Bird Migration at Ann Arbor.'

Norman A. Wood: 'The Bird Life of Ann Arbor, Michigan, and Vicinity.' (By title.)

E. H. Frothingham: 'Notes on the Birds of the Michigan Forest Reserve.'

R. A. Brown: "A Topographical Study of the Birds of the 'Overflow,' at Ann Arbor, Mich."

Chas. C. Adams: 'An Ecological Survey of Isle Royal, Lake Superior.'

Otto McCreary: 'The Ecological Distribution of the Birds on Isle Royal.'

Max M. Peet: 'The Fall Migration of Birds on Isle Royal.' (By title.)

Professor Walter B. Barrows, president of the academy and of the club, gave his presidential address before the academy, on 'Facts and Fancies in Bird Migration' in the new lecture room of the physical laboratory on Thursday evening.

A business meeting was held in the afternoon in the office of the curator of the university museum. The following officers were elected for 1906–7.

President—Walter B. Barrows, Agricultural College.

First Vice-president—J. Claire Wood, Detroit.

Second Vice-president—Edward Arnold, Battle Creek.

Third Vice-president—Norman A. Wood, Ann Arbor.

Secretary—Alexander W. Blain, Jr., Detroit.

Treasurer—Frederick C. Hubel, Detroit.

Editor of the Bulletin—Walter B. Barrows.

Associate Editors—Wm. H. Dunham, Kalkaska; R. A. Brown, Kalamazoo.

The meeting adjourned to meet at the Detroit Museum of Art on May 4, 1906.

Alexander W. Blain, Jr.,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE FALLACY OF THE MUTATION THEORY.

Dr. C. H. Merriam has lately pointed out[1] that *mutation* in de Vries's sense is not a species-forming factor, and that it is rarely, if at all, observed among living animals. Major T. L. Casey objects[2] to this sweeping condemnation of de Vries's theory, and believes that there 'may be a good deal' in the latter.

I only can endorse Merriam's view, and want to go on record as condemning even more emphatically the mutation theory for the following reasons:

De Vries claims that the process of mutation forms new species, and that the individual mutations (mutants) *are* species. In order to demonstrate this, he has made a number of experiments, in which he tries to show that the mutations breed true, and he uses this fact as a *test* for the specific value of the mutations. No other test is admitted, or even mentioned, by him.

This shows at a glance that de Vries's conception of the term species is all wrong, that he does not know what constitutes a species, in spite of his lengthy discussion of this term. Of course, it is generally admitted that species should breed true: but this is also a necessary character that belongs to the concept of variety. What distinguishes species from varieties is the fact that a species is not connected by intermediate or transitional forms with the most closely allied species. This latter principle is the one made use of exclusively (if possible) by systematists, botanists as well as zoologists. In many cases, indeed, it can not be used on account of the insufficiency of our knowledge; but under such conditions new species are always described with the tacit understanding that the demonstration of the existence of intermediate forms will reduce them to the rank of varieties.

De Vries has failed entirely to take notice of this fundamental principle, and to show that his elementary species and his mutations are *not* connected by intermediate forms with each other. But looking over the instances introduced by him, we see that such intermediate forms are recorded by de Vries himself, and I know from personal experience that such are present among several of the polymorphous genera mentioned by him (*Viola, Draba*).

Further, according to the experimental records on *Œnothera,* given by de Vries, I can not see how he is in a position to main-

[1] Science, February 16, 1906, p. 241, chiefly pp. 256 and 257.

[2] Science, April 20, 1906, p. 632.

tain that the mutations have bred true. They surely did not do this in the beginning of the experiments, since they were throwing off, in each generation, additional mutants, and it was only after some time that de Vries succeeded in obtaining a relatively pure strain.

Consequently, de Vries's contention that mutations are species is not supported at all by his experiments; whatever they are, *they are not species, since they do not show the characteristic features of such.*

However, if de Vries had claimed that species might be made out of mutations, nothing could be objected to this view; but this is no new idea. Similar experiments have been undertaken by animal and plant breeders, and a large number are on record. In fact, the breeding of domestic races has always been regarded as a process analogous to the one in nature by which new species are produced. But the main features of this process in nature as well as under domestication are *selection* and *segregation*. This is exactly what de Vries has done with his mutations: he selected and segregated them (preventing crossing), and thus he imitated nature's way, and finally obtained more or less pure strains, which are analogous to natural species. But before he began this process of selecting and segregating, the mutations were by no means species, but only varieties.

Aside from the above claim, de Vries further maintains that it is the mutations, and not the variations, that give origin to new species, and he thinks that there is a fundamental difference between them. However, I have been unable to see where he draws the line between variations, constituting small steps, and mutations representing sudden leaps, and I do not think that he has solved the old sophistic problem of how much must be added to a small thing in order to make it a large one. His discussion of unit-characters does not offer any help in this respect, since in many cases he confesses himself that he does not know what should be regarded as a unit-character.

Mutations are by no means as frequent as de Vries would fain make us believe. He concedes himself that he had considerable trouble in finding a fit object for his experiments, and, indeed, among living animals and plants in the wild state, mutations in de Vries's sense are extremely rare, and in this respect I agree entirely with Merriam's contention, not only with reference to animals, but also to plants. True mutations, that is to say, variations which represent sudden leaps, are found chiefly among domestic forms, and this fact, I think, is well established; and the form that finally furnished the material for de Vries's experiments, *Œnothera lamarckiana,* is a domesticated, a garden form, and not a native species of Europe. It is true, it lately has become a habit with some biologists to hunt for mutations in nature, but the search has been quite unsuccessful, for the so-called mutations in part do not at all represent sudden leaps; in part it was not considered worth while to investigate whether the sudden leaps discovered were connected with the original form by transitions or not.

Paleontological evidence for the former existence of mutations should be excluded from the beginning, since it is in the very nature of paleontological facts to be fragmentary, and, in this connection, it is well to call attention to the former use of the term *mutation* by paleontologists (Waagen, Neumayr, W. B. Scott); it means just the opposite of de Vries's mutation, namely, a change during phylogenetic development, which is characterized by slow, small, almost insensible steps. For this we possess *positive* proof.

Thus we arrive at the conclusion that de Vries has not made good his claim that mutations are species, since his conception of species is defective. If he should change his view, and claim that species could be made out of mutations, he would be right, but then it is the selection, and chiefly the *segregation,* that has this effect; and further, this would be no new theory. If he claims that it is mutation as distinguished from variation that starts the species-forming process, we must point out that mutations are rare in nature, that there is no sharp line to be drawn between mutation and variation, and that mutation has always been regarded as a special form of variation (sporting, halmatogenesis). Con-

sequently, nothing is left of de Vries's mutation theory but the bare facts represented by his experiments, which, indeed, are valuable for the study of variation, but belong to a class that was already known to Darwin when he wrote his 'Origin of Species' and 'Variation under Domestication.' For the rest, I do not see that there is anything in the mutation theory which might advance our general knowledge of the factors cooperating in evolution.

A. E. Ortmann.

Carnegie Museum, Pittsburg, Pa.,
April 26, 1906.

MISREPRESENTATIONS OF NATURE IN POPULAR MAGAZINES.

From the numerous and conspicuous mistakes made by the popular magazines when treating of geographical and geological subjects it would appear that there is occasion for more careful editing by men conversant with scientific affairs.

Many of the mistakes are more than simply inaccuracies of statement or occasional exaggeration. They are often the most conspicuous thing in the magazine.

Take, for example, the finely colored full-page picture in the *Century* (Vol. XLVII., p. 553) entitled 'Sulphur Deposits at the Crater Vesuvius.' The fact is that there are no sulphur deposits at Vesuvius. Not only are there no deposits, but even a trace of sulphur is difficult to find. Unless the volcano changes its chemistry to accord with the *Century* there will be none from this last eruption. The artist evidently mistook the lava which had been bleached by chlorine to be sulphur; the editor allowed the mistake to pass; and all who gain their idea of Vesuvius from that source will have much to unlearn when they hear the facts.

The Outing Magazine, edited by men who have more than an indoor acquaintance with nature, begins this year with a frontispiece (January number) entitled 'Bridger was the first man to gaze on the Great Salt Lake' and represents Bridger standing on the shore while his horse, with nose deep in the lake, is eagerly drinking! We have seen many wonderful bronchos, but never one that could drink the water of Salt Lake.

A well-written article in *McClure's* (Vol. XXV., p. 504) is illustrated by many pictures of the Grand Cañon of the Colorado. The coloring was evidently done by one who had never seen the region. It entirely misrepresents the cañon and must have annoyed the artist. But even the drawing gives a wrong impression of the greatest of cañons, just as would a picture of Broadway or of State Street which represented the high buildings sloping towards each other across the street. There are no narrow gorges in the cañon such as those pictured. This style of illustration is a recurrence of the type of picture furnished by Egloffstein in 1857 for the Ives Report published by the United States government. It was hoped that misrepresentations of that character would end with that century.

Nature is as interesting and impressive as are exaggeration and misrepresentation. A picture may have the educational value of many pages of sentences, since it so readily catches the eye. Many people will see a picture, while few read the text. Consequently it is important that pictures should represent facts and it behooves the popular magazines to have not only careful literary, but scientific editing as well.

A. R. Crook.

ALLUVIAL SLOPES.

One of the commonest topographic features of the western part of the United States, particularly of the arid west, is the characteristic sloping plain which fringes the flanks of the mountain ranges and is formed by coalescent alluvial fans. Many terms have been used to denote this sloping plain, among which are: alluvial slope, alluvial apron, alluvial piedmont plain, compound alluvial fan, wash apron, débris apron, detrital slope, wash plain, out-wash plain, foot slope, aggradation plain, boulder wash plain and others. It seems desirable that such a typical feature should bear a more specific appellation. The consensus of opinion of the geologists of the United

States Geological Survey, as recently expressed, appears to strongly favor the use of *alluvial slope*, which thus takes its place in the genetically related series, alluvial fan, alluvial slope and alluvial plain.

C. E. SIEBENTHAL.

SPECIAL ARTICLES.

THE NORTHERN LIMIT OF THE PAPAW TREE.

WHILE the flora of the upper Mississippi Valley was yet in its primeval condition I had good opportunities to observe the northward geographical extension, and apparently the northern limit, of certain plants which reach full maturity of growth and fruitage farther south. Although the floral conditions which then existed in that region have been in part modified by the progress of civilization, the chief of the following statements are based upon conditions which still exist. Among the plants referred to is the papaw, *Asimina triloba*, which reaches its maximum size, that of a small tree, and its most abundant fruitage in the broad region of which the mouth of the Ohio River is near the center. Two of the many other trees which are associated with it there and which have accompanied it in their northward dispersion, are selected for special comparison. These are the persimmon, *Diospyros Virginiana*, and the pecan, *Carya olivæformis*.

It is of relevant interest to note that although these three species are commingled in the same flora in the valley of the upper Mississippi, their post-glacial dispersion into that region seems to have been from a pre-glacial flora which occupied the papaw area before mentioned and which was in part made up of trees from different districts. The districts which thus furnished the persimmon and pecan respectively are assumed to have been identical with their present respective areas of greatest abundance and fruitfulness. That is, the center of the area of greatest abundance and fruitfulness of the persimmon may be designated as within the state of Virginia, which is far east of the similar center of the papaw area, while it is in southern Texas and the adjacent part of Mexico, far from both the persimmon and papaw centers, that the pecan reaches its greatest abundance and perfection. The persimmon apparently spread westward into the papaw area and thence northward; while the pecan ranged up into the Mississippi Valley, traversing the papaw area, and thence at least as far north as the forty-first parallel of north latitude, a thousand miles from the region of its fullest development. The unaided dispersion of the papaw seems to have been proportionately less than that of either the persimmon or pecan.

In 1846, pecan trees of moderately large size were yet growing and bearing fruit in fair abundance ten miles above Burlington, Iowa, and a number of persimmon trees of moderate size were also then growing and bearing fruit in its season a few miles below Burlington, on the Illinois side of the great river. Neither of these two trees was then common in that region and, so far as I could ever learn, the localities mentioned constituted the northern limit of their dispersion. It is pertinent to my present purpose to mention that both of those trees retained their fruiting function unimpaired in their most northerly extension, although the case was very different with the papaw. During many years I observed the last-named plant growing as a part of the local flora at numerous localities along the banks of the Mississippi, from northeastern Missouri to the forementioned locality north of Burlington, where the pecan grew, the distance between the two extreme localities being about seventy-five miles. In the southernmost of the Missouri localities referred to the plant reached almost arborescent size and frequently, but never abundantly, bore and matured its fruit. From there northward, however, although the vegetative growth of the plants was apparently healthful, they never fruited, and gradually diminished in size to shrubs, a few feet in height. It is true that some thrifty specimens which grew upon the Iowa bank of the Mississippi River, a few miles above Keokuk, occasionally flowered, but, although I frequently examined them in the flowering and fruiting seasons, I could never find any evidence that fruit had been formed, or that any ovaries had ever become

fertilized. Northward from that locality to its most northerly one, a distance of fifty miles, the papaw plants seldom flowered, and if one occasionally bore flowers, the ovaries never matured.

Here, then, is the case of a plant losing its power of reproduction from some natural cause, but still growing thriftily at short intervals along a stretch of more than fifty miles beyond its fruiting limit. It is not necessary to my present purpose to inquire into that cause, but the unfruitfulness of the plant was no doubt due to the failure of one or more of the various devices which nature provides for the pollination of flowers; and that condition, as well as the diminishing stature of the plant toward the north, was perhaps correlated with climatic change. This case of the papaw, however, is so unusual that one naturally wonders how that northern portion of its geographical dispersion could have been accomplished without the aid of the function of reproduction at every stage of its progress. It is necessary to assume that every one of those unfruitful papaw plants within the range mentioned originated from seed which was brought from a southward locality by some biological, and not a physical, agency. That is, the transportation could not have been effected by either air or water currents because the large, oblong, flattened seeds of the papaw are too heavy to be borne by the wind, and all the fluviatile currents of that region are in an opposite direction. It is quite improbable that the seeds were transported by either birds or quadrupedal animals, because, while the ripe fruit-pulp would be greedily eaten by some of them, the seeds are evidently unsuited for the food of any; and because no frugivorous birds or other animals of that region have the migratory and garnering habits which such a suggestion requires. Moreover, the papaw ripens its fruit when southward, and not northward, bird-migration is impending. It, therefore, seems necessary to assume that the transportation of the seeds was effected by human agency; and because one fails to see how their germination and growth in those northern localities could have been advantageous to any human interests it further seems necessary to assume that their distribution was incidentally, but not intentionally, connected with the nomadic habits of the Indians. This suggestion is not wholly satisfactory when one considers only the ordinary conditions of Indian life, because the fresh, ripe fruit was never too abundant in any part of that region for immediate use, it is not suitable for preservation in camp, and the seeds have no obvious value. I, however, casually discovered what seems to be a sufficient explanation of the manner of transportation of those seeds and of the circumstances of their germination beyond the geographical limits of the natural fruiting of the species.

The Sac and Fox Indians were originally in possession of both banks of the Mississippi River from the confluence of the Missouri River to above that of Iowa River. This region includes the continuous range of the papaw from where it reaches full fruitage to the northernmost limit of its merely vegetative growth which already has been mentioned. Those Indians were in the habit of ranging all that portion of the great river in their canoes and of camping upon its banks. Upon one of my excursions I came upon one of their river-bank camps, when the men were away fishing or hunting and a small group of women were sitting in a circle on the ground playing a game which consisted of tossing a small number of papaw seeds in a basin. The hard, smooth seeds each bore a distinguishing artificial mark, upon one side only, the difference in the form of the marks as they appeared after each throw, evidently indicating differences of value in counting the game. As I and my companion stopped to watch the game I said to him 'those are papaw seeds.' One of the women looked up with a smile—she was perhaps a winner in the game—and said, 'yes, papaw,' and pointed to a quantity of the seeds that lay on a garment near her, evidently their stock for future games. Without doubt that game had been practised by the Sac and Fox Indians for centuries, and it is easy to understand how they could have procured the seeds by their southern journeys,

how a few of them might become lost at any camping place, and how they might germinate there by natural means.

It is true that all the floral districts of the earth contain plants the migratory introduction of which has been caused by the agency of man. As a rule, those plants are self-perpetuated in their new habitats by their unimpaired function of reproduction; and if they have lost that function from any cause their preservation is due to man's intervention for his own benefit. In the case of the papaw here mentioned its vegetative growth has extended far beyond its fruiting limits, a condition which savage man could have no interest in preserving. His gambling habit seems, therefore, to have been the accidental cause of that part of the dispersion of the papaw which it would not have attained by merely natural causes.

CHARLES A. WHITE.
SMITHSONIAN INSTITUTION,
April 21, 1906.

THE PARASITISM OF NEOCOSMOSPORA.

A WILT disease was discovered by the writer in the ginseng gardens of Missouri in the summer of 1904, which proved to be caused by the fungus *Neocosmospora vasinfecta* var. *nivea* Sm. The fungus has been studied and described in a bulletin soon to be issued by the Missouri Agricultural Experiment Station. In the course of the investigation several new facts were ascertained concerning the parasitism of the fungus, which may properly be mentioned in this place.

The characters of this fungus have been studied principally by Atkinson and Smith. The former[1] first described the fungus in 1892, believing it to be a species of *Fusarium*. He stated the belief that it was a weak parasite, since it usually infected only such plants as had been previously attacked by another disease. Smith,[2] in 1899, published a detailed account of the entire life history of the fungus, giving it the name *Neocosmospora vasinfecta*. He found that there were three distinct physiological varieties which attacked cotton, watermelons and cow-peas, respectively, and that cross-inoculations always failed. In contrast to Atkinson's assumption, Smith stated that all three varieties were parasitic, and especially the variety *nivea*.

Although I failed to obtain the perithecia, yet in all other respects the ginseng fungus agreed with the variety *nivea* (the watermelon fungus of Smith). The results related in the bulletin soon to be issued go far toward establishing Atkinson's theory that *Neocosmospora* is a weak parasite and only attacks plants which are first weakened by the presence of another fungus.

The facts upon which this conclusion is based are as follows: (1) In the field the wilt disease never appeared except where the ginseng plants had been previously attacked by an anthracnose. (2) Plants which were sprayed with the Bordeaux spraying mixture (and consequently free from anthracnose) were not attacked by *Neocosmospora*. (3) Watermelon seeds were planted in crocks of rich garden earth which certainly contained microorganisms, but had never been infected with *Neocosmospora*. Each crock received a test-tube culture of *Neocosmospora* at the time of planting seeds and three weeks later the melon seedlings were attacked by the wilt fungus. Microscopical examination of the wilted seedlings showed the pink mycelium and spores of *Neocosmospora* in the fibrovascular bundles of the hypocotyls. Other crocks filled with the same kind of soil were sterilized by steam in an autoclave. When cool they each received a tube culture of *Neocosmospora* and were planted with watermelon seeds. The wilt fungus grew abundantly in the sterilized soil, but at the expiration of twelve weeks none of the watermelon plants showed the slightest indication of the wilt disease.

These facts are interpreted to mean that *Neocosmospora* itself is a weak parasite, but when associated (as it usually is) with other fungi, *e. g.*, *Rhizoctonia*, *Pythium*, etc., it gains entrance into the watermelon plant. In the case of ginseng, its entrance seems to depend upon an anthracnose caused by *Vermicularia Dematium*.

[1] Bul. 41, Alabama Agr. Exp. Sta., 1892.

[2] Bul. 17, Div. Veg. Phys. and Pathol., U. S. Dept. Agr., 1899.

Smith's experiments with *Neocosmospora* employed soil which was uninfected by that fungus, but apparently was not sterilized, and he states that *Thielavia basicola* was present in some of the experiments. To this fact I am inclined to refer the apparent active parasitism which he found. At any rate, the form which I have isolated from the ginseng plant has not shown active parasitism.

Howard S. Reed.

Botanical Laboratory,
University of Missouri,
March 15, 1906.

EFFECTS OF AN UNBALANCED RATION.

In 1904 an obscure disease affected the fruit of certain trees in the orchard of the Maine Agricultural Experiment Station. No similar trouble had previously come under the notice of the writer, and this note is made simply as a matter of record. A careful study of the cause of the condition described is being carried on at the present time.

In August, when about the size of walnuts, the fruits began to crack and drop. Marked indentations, somewhat similar to those made by curculio, were abundant. No evidence of insect work could be discovered, however. When the fruit was opened the tissue under the indented parts was found to be dry and brown. Most of the fruits ceased to grow, and by the first of September the larger part of it was on the ground; though early in the season all of the trees were loaded. The leaves, however, appeared perfectly healthy.

At the time of harvesting, October 10, most of the trees had lost all of their fruit. Such as remained on some of the trees was usually small and deformed; some was of medium size with one side cracked; and a small portion was without blemish. In all cases, however, the texture of the fruit was soft and spongy—about as might be expected in April or May. The surface of the fruit was also characteristic; there being numerous minute elevated 'pimples,' corresponding to the grayish dots on the fruit. This feature was so noticeable that the workmen spoke of it in handling the fruit after removal to the cellar.

Though a small portion of the fruit was on the tree at harvest time, it dropped so easily that no attempt was made to save it for packing. The slightest jarring of the limbs would cause it to fall.

The reason for the condition above indicated is, as already noted, very obscure. A careful microscopic examination was made without finding evidence of any fungous enemy, even in the brown dry tissue above mentioned. It was then observed that the condition existed only with certain trees included in a fertilizer experiment, in which an excess of available nitrogen is applied every year. The first tree noticed was on the plat receiving nitrate of soda and acid phosphate. Later it was found that every tree on the plat, as also on the adjoining plat which received nitrate only, was affected as described. In one or two instances check trees, which adjoined the nitrate plat, showed a tendency in this direction. None of the other trees in the whole orchard, however, gave the slightest indication of the trouble. The trees on a plat given muriate of potash and acid phosphate, and on another given muriate only, separated from the first by but a single row of trees, were perfectly normal.

The supposition was made, therefore, that the trouble was physiological and due to the excessive amount of available nitrogen and the lack of potash. Of course this is a matter of conjecture and can be settled only by definite and careful experiment.

The outcome of a further study of this problem may be of interest and importance in connection with the rational fertilization of orchards.

W. M. Munson.

University of Maine.

NOTES ON ORGANIC CHEMISTRY.

THE ACTION OF OZONE ON ORGANIC COMPOUNDS.

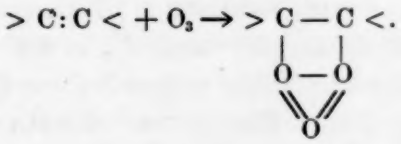
Since the discovery of ozone by Schoenbein, in 1840, many chemists have examined its action on organic compounds, but, hitherto, with very limited success. When a reaction did take place it resulted, almost always, in the formation of carbon dioxide and water, or in the production of highly explosive sub-

stances which were not suitable for investigation.

Some time ago Professor C. Harries, in the course of an investigation of caoutchouc, studied its behavior towards ozone. The results were not very conclusive, but they led him to take up the wider question of the interaction of ozone with organic compounds in general. He has published various brief papers on the subject, from time to time, during the interval and quite recently he has collected his results into a longer article[1] which is of considerable interest.

The first requisite was the provision of an adequate supply of ozone, of relatively high concentration, and for this purpose a special form of generator was devised by Siemens and Halske. The experiments were conducted as follows: The substance under investigation is dissolved in chloroform, or low boiling ligroin, the solution cooled in a freezing mixture and ozonized oxygen passed into it. To minimize the danger of explosion a current of carbon dioxide is directed through the apparatus, which must be thoroughly well dried; moisture in the materials must also be rigorously excluded.

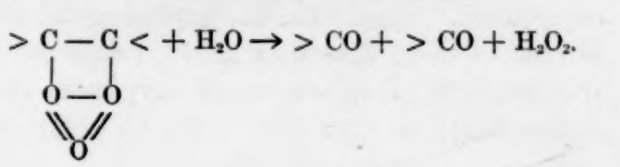
The experimental results lead to the conclusion that all organic compounds containing an ethylene linkage (double bond) add one molecule of ozone, giving rise to a new class of substances termed ozonides:



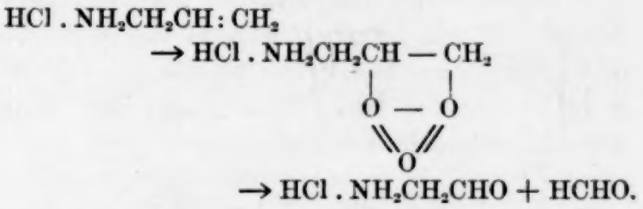
It will be observed that Harries formulates the oxygen as tetravalent; he is, however, not wholly consistent on this point in his paper and, in any case, the results can be just as accurately expressed by the bivalent formula. The ozonides are viscid oils, or colorless, vitreous syrups, more or less explosive and possessing an unpleasant, suffocating smell; some of them can be distilled under reduced pressure. They liberate iodine from potassium iodide and bleach indigo solution.

[1] *Ann. Chem.* (Liebig), **343**, 311.

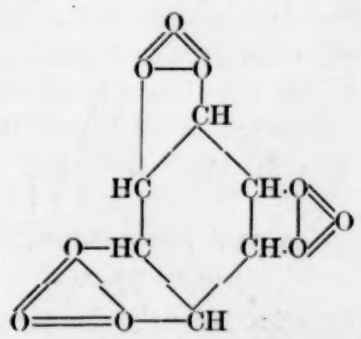
The most interesting reaction of the ozonides is with water, with which they yield hydrogen peroxide and aldehydes or ketones:



Thus, from allylamine hydrochloride, the corresponding salt of aminoacetaldehyde is obtained:



This method of work has already proved its value in permitting the comparatively easy preparation, in fairly large quantity, of many substances which were hitherto unknown, or could only be obtained with the greatest difficulty. Another, equally valuable feature of the research, is the application of the reaction to the determination of the constitution of compounds. Of many of the examples given in the original paper, one must suffice for quotation. According to Kekulé's formula benzene has three double bonds, whereas, according to the 'centric' formula it has none. If the former be correct benzene should add 3 molecules of ozone and the resulting product, when treated with water, should yield glyoxal. This is exactly what really happens, the triozonide,



which is first formed, yields, with water, 3OCHCHO. This constitutes one of the strongest, purely chemical supports for the Kekulé formula which has been discovered for many years.

The action of ozone on carbonyl derivatives (aldehydes or ketones) is hardly less interesting than its behavior towards unsaturated compounds. Under similar experimental conditions to those described above, ozone causes the addition of one atom of oxygen to each carbonyl group.

$$> CO + O_3 \rightarrow > C{:}O{:}O + O_2.$$

These substances, which are peroxides, readily react with water forming hydrogen peroxide and regenerating the original carbonyl compound. It will be observed that the peroxides of aldehydes are isomeric with the acids which may be derived from these aldehydes; thus, heptoic aldehyde (œnanthol),

$$CH_3CH_2CH_2CH_2CH_2CH_2CHO,$$

yields the peroxide, $CH_3(CH_2)_5CH{:}O{:}O$, which is isomeric with heptoic (œnanthic) acid, $CH_3(CH_2)_5COOH$. It is found that such peroxides change spontaneously, after some time, into the corresponding acids, a fact which may have an important bearing on the theory of the oxidation of aldehydes to acids.

In the case of carbonyl compounds containing one or more ethylene linkages both the above reactions occur simultaneously, *i. e.*, one molecule of ozone is added to each double bond and one atom of oxygen to each carbonyl group.

Harries finds that ether appears to be capable of absorbing unlimited quantities of ozone; the product is very highly explosive. These facts probably account for the mysterious ether explosions which occur in laboratories from time to time; ozone is produced during many reactions, such as occur in flames, is absorbed by ether and the explosion follows when the ether is heated.

RESEARCHES ON THE AMINO ACIDS, POLYPEPTIDES AND PROTEIDS.

Emil Fischer recently delivered a lecture, under the above title, before the German Chemical Society. The report of the lecture occupies more than eighty pages of the 'Berichte'[1] and contains the chief results of his labors in the field in question during the past six years. It thus represents the cream of several dozens of papers, and although it is severely compressed, yet the author's name is sufficient guarantee that the style is fascinating and the material of the highest order of merit. In this brief note it is, obviously, impossible to do justice to the lecture, yet some of Fischer's results and conclusions are of such general importance that a brief outline of them should be given.

The hydrolytic cleavage products formed by the action of acids, bases, or enzymes on proteids are chiefly mixtures of various amino, hydroxyamino and diamino acids, belonging to the aliphatic series, although similar derivatives of the alicylic and aromatic series are not infrequently found. Fischer's first task was, therefore, to improve the methods for the synthesis of the above classes of compounds and to investigate their individual properties, so as to work out methods for the separation and identification of each substance when present in the mixtures already mentioned.

The second step consisted in the synthesis of 'polypeptides,' which are amide anhydrides of amino acids. Thus aminoacetic acid (glycocoll, glycine), H_2NCH_2COOH, yields the amide anhydride $H_2NCH_2CONHCH_2COOH$, which is termed glycylglycine and is a *dipeptide*. No limit can, as yet, be assigned to the number of such compounds which may be prepared; they usually crystallize well and are, therefore, admirably adapted for purposes of investigation. The most complicated one which has been obtained up to the present time is a heptapeptide.

Synthetical polypeptides are, of course, racemic, but those which are obtained from proteids frequently exhibit optical activity. Fischer has worked out several methods for the resolution of his racemic compounds into their optical antipodes. The results are in complete accord with the current theory of the asymmetric carbon atom.

Fischer's efforts have not been confined to synthesis; he has also investigated the compounds formed by the resolution of some

[1] *Ber. d. Chem. Ges.*, **39**, 530 (1906).

twenty of the more important proteids. Here the methods of purification referred to above have done admirable service, and their application has thrown much light on the products of the reactions in question. In this part of the work the 'esterification method' has proved to be of the greatest value. The mixture of crude amino acids is treated with alcohol and dry hydrogen chloride, and the product fractionated under highly reduced pressure (0.5 mm.).

Experiments on the peptones and albumoses have been made, but the results are not very definite; there is scarcely room for doubt that the substances obtained by the ordinary processes, such as precipitation from saline solutions, are not pure, individual compounds, but are mixtures, and new methods will be required for their satisfactory treatment. The peptones are probably complex mixtures of polypeptides.

Of the structure of these compounds little can be said with certainty at present, and practically nothing of that of albumin, for the fruitful investigation of which entirely novel methods will have to be discovered.

The number of new compounds which Fischer has obtained is already very large, and the thorough application of his methods will enormously increase it and will require the work of numerous chemists for many years. Of course this only represents a fraction of the actual quantity of work necessary, because new methods of synthesis and new combinations will undoubtedly be discovered. The question, therefore, arises, will the probable benefit be worth such a large outlay of time and money? There can be no hesitation in answering this in the affirmative. Only by the painstaking, careful investigation of all possible permutations and combinations of the polypeptides and their cogeners, can a firm foundation be laid for a real knowledge of the proteids and related compounds such as enzymes, toxins, etc., the importance of which to the biologist it is difficult to overestimate. The point may be put in another way: Suppose a true proteid were to be synthesized immediately, in some such simple—and brutal—way as the heating of an amino acid in presence of a dehydrating agent, what would be gained? The answer is, practically nothing for biology and hardly anything more for chemistry.

All interested in science will join in the hope that Professor Fischer's restoration to health may be sufficiently permanent to enable him to continue, for many years, his wonderful and valuable investigations.

J. BISHOP TINGLE.

JOHNS HOPKINS UNIVERSITY.

THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS AND THE METRIC SYSTEM.

THE following preamble and resolutions concerning the 'metric system' were favorably reported to the board of directors of the institute by the standardization committee at the directors' meeting of March 23, 1906:

WHEREAS, The Metric System of Weights and Measures offers very great advantages by its simplicity, consistency, and convenience in every-day use, as well as in engineering and scientific calculations and computations; and

WHEREAS, These advantages have already been demonstrated by the universal adoption and entirely successful use of the metric system in all civilized countries, except Great Britain and the United States; and

WHEREAS, All the electrical units in universal use, such as the volt, ampere, ohm, watt, etc., are metric units; and

WHEREAS, The industrial use of these electrical units would be much facilitated by the general adoption of the metric system.

Resolved, That this committee unanimously recommends the introduction of the metric system into general use in the United States at as early a date as possible without undue hardship to the industrial interests involved.

Resolved, That this committee favors such legislation by Congress as shall secure the adoption of the metric system by each department of the national government as speedily as may be consistent with the public welfare.

The board subsequently submitted the matter to the membership for a letter ballot. Of the 3,300 associates and members residing in the United States, replies were received from 1,747 up to May 5. Of these 1,569 voted in favor of the resolutions and 178 against them.

THE CALIFORNIA EARTHQUAKE AT UKIAH.

THE earthquake which wrought such destruction in San Francisco and Santa Rosa, on April 18, was very severe in Ukiah, 160 kilometers (96 miles) northwest of San Francisco. Many chimneys were thrown down from two-story buildings but cottages escaped without injury. One new brick store building, just nearing completion, was so badly cracked and thrown out of plumb that it is necessary to tear it down.

At the Latitude Station no damage whatever was done although the shaking was the most severe ever experienced by the writer. Dishes rattled, milk was spilled from pans but little more than half full, and fowls and other domestic animals were very much perturbed. There were a series of shocks and reliable estimates of their duration vary from twenty seconds to one minute. The general direction of the wave seemed to be from southwest toward the northeast, but others report a different direction. The Ukiah Valley is surrounded by mountains of considerable altitude and it is probable that some of the shocks felt were from waves reflected from the mountains. Hence it is that the earthquake is generally spoken of as a 'twister.'

The observatory clock was not stopped but it lost six seconds during the disturbance, which is equivalent to being stopped for that length of time and then set to going again. The observatory roof is built in two sections which roll upon horizontal tracks, east and west, giving an opening for observation of about 1.8 meters. When closed the two parts are fastened together by means of a hook and eye such as are used on screen doors. The hook rests in a horizontal position and the bend of the hook in a meridian plane. The effect of the earthquake was to unfasten this hook and open the roof to a width of about twenty centimeters, my recollection being that the eastern half was moved about twice as far as the western. The pier upon which the zenith telescope rests is apparently not damaged but the telescope was thrown considerably out of adjustment. It was out about fifteen seconds of arc in azimuth and the vertical axis was out in both directions, but not much more than sometimes results from extreme changes in temperature.

The first series of shocks was followed by three lighter ones and the observed data for each are as follows:

Pacific Stand. Time 1906 April					Duration.	Direction.	Intensity.
18d	5h	13m	00s	A.M.	About 40s	S.W. to N.E.	Severe.
18	10	4	39	A.M.	" 10	S.W. to N.E.	Medium.
18	11	36	00	A.M.	" 30	S.W. to N.E.	Light.
20	12	30	53	A.M.			Very slight.

The times are correct within two or three seconds.

I was in the observatory at the time of the second series of shocks, at $10^h\,4^m$, and perceived the effect of the movement in the striding level (east and west), of the zenith telescope. The bubble oscillated over about two divisions of the level. The value of one division is 2".2 and as the distance between the east and west leveling screws of the instrument is about 42 centimeters, the disturbance produced in the bubble was equivalent to the effect of raising and lowering one of the leveling screws by 0.0005 centimeter. This shock was felt very distinctly and it is probable that the north and south component of the motion was much greater than the east and west one.

The fourth shock was not felt at all. It was detected during the progress of latitude observations by a movement of the bubbles of the latitude levels. The oscillation (north and south) was about one half of one division, and the value of one division is 1.0.

SIDNEY D. TOWNLEY.

INTERNATIONAL LATITUDE OBSERVATORY,
UKIAH, CALIFORNIA.

SCIENTIFIC BUILDINGS AND COLLECTIONS AT STANFORD UNIVERSITY.[1]

THE scientific laboratories and collections at Stanford University were but slightly injured by the recent earthquake in California. The buildings containing the departments of physiology, botany, zoology and entomology are uninjured structurally, and the apparatus

[1] We print this note, although it reduplicates to a certain extent the information communicated to us last week by President Jordan.

and collections suffered almost no damage. The chemistry building lost small parts of two walls, and the loss to apparatus and supplies amounts to a few hundred dollars. The present geological and metallurgic laboratories are, with their contents, practically unhurt. The large new geological building, nearly completed, suffered serious injury. The building in which the departments of physics and psychology are housed lost a part of one wall, but the equipment is but slightly damaged. The laboratories and shops of the various engineering departments show some injuries, all of which, however, can be easily and quickly remedied. The really wrecked buildings are the famous church, great memorial arch, museum and the large new library and gymnasium buildings in course of erection. University work will begin again (it has been suspended for the rest of the present semester, about four weeks) on August 23, the regular date for the opening of the next college year.

VERNON L. KELLOGG.

THE UNIVERSITY OF CALIFORNIA AND THE CALIFORNIA ACADEMY OF SCIENCES.

THE University of California suffered by the loss of San Francisco investments, but the buildings and their contents at Berkeley suffered very little damage. Academic work, interrupted for the present by relief work, in which nearly all of the members of the faculty are engaged, will shortly be resumed. We understand also that the buildings of the affiliated colleges in San Francisco were not seriously injured. The Anthropological Museum is saved, and the building is undamaged, the loss to the collections is inconsiderable and altogether from earthquake. The most fragile pieces of value were efficiently protected by appliances designed against earthquake shocks. The university's Mark Hopkins Institute of Art in San Francisco was burned, but nearly all canvases were saved. The Bancroft library of books and manuscripts relating to the history of the Pacific coast, which recently came into the possession of the university, has been saved.

Among the direct losses by the fire which followed the earthquake in San Francisco were the building, library and natural history collections of the California Academy of Sciences. The building was materially injured by the earthquake, its staircase in particular suffering severely, but this did not prevent an effort to rescue some of the more precious material before the fire reached it. A small party of curators and members climbed to the laboratories and library on the upper floor and brought away the type specimens in botany, entomology and herpetology, together with some manuscripts and the archives.

SCIENTIFIC NOTES AND NEWS.

THE American Chemical Society will meet at Ithaca, N. Y., June 28–30. The following persons have been appointed as chairmen of the various sections:

Physical Chemistry, W. Lash Miller.
Inorganic Chemistry, L. M. Dennis.
Organic Chemistry, G. B. Frankforter.
Biological Chemistry, Waldemar Koch.
Agricultural Chemistry, E. B. Voorhees.
Industrial Chemistry, J. D. Pennock.

A MEETING of those interested in the organization of an American association, similar in scope to the Museums Association of Great Britain, will, as we have already reported, take place at the American Museum of Natural History, New York, on May 15, at 10:30 o'clock. A large number of men from various parts of the country have expressed their intention of being present, and at the conclusion of the business of organization, it is proposed to attend to the reading of papers upon various museum subjects. On Tuesday luncheon will be served at the American Museum, and on Wednesday sessions will be held and luncheon will be served at the Botanical Garden. It is suggested that those who are strangers in New York will find convenient quarters at reasonable rates at the Hotel Endicott, corner of Ninth Avenue and Eighty-first Street.

THE Rumford committee of the American Academy has recently made grants to the following persons in aid of the researches specified:

Arthur B. Lamb, of Harvard University. Specific Heat of Salt Solutions..... $200
John A. Parkhurst, of the Yerkes Observatory. For the purchase of a Hartmann photometer 225
Professor Charles B. Thwing, of Syracuse University. Thermo-electric Power of Metals (second appropriation)..... 400
Professor Edwin H. Hall, of Harvard University. Thermo-electric Properties of Metals 125
Professor Frederic E. Kester, of the Ohio State University. Joule-Thomson Effect in Gases..... 50

Dr. Henry Pickering Bowditch, George Higginson professor of physiology in the Harvard Medical School, has sent in his resignation to take effect at the end of the current academic year. Dr. Bowditch graduated from Harvard College in 1861 and from the Medical School in 1868. He was appointed assistant professor of physiology in 1871 and professor in 1876. We regret to learn that Dr. Bowditch's resignation is due to ill health.

Professor W. Ostwald, of Leipzig, has been elected a foreign member of the Danish Academy of Sciences.

Professor Lewis M. Haupt, A.M., Sc.D., delivered a lecture on 'The Panama Problem,' on May 3, before the students of Muhlenberg College and the citizens of Allentown, Pa. He will address the Engineering Department of the University of Michigan on the subject of 'The Emancipation of the Waterways and Commercial Highways' the latter part of this month.

Professor Israel Cook Russell, head of the Department of Geology at the University of Michigan, died on May 1, of pneumonia after a brief illness. He was born at Garrattsville, N. Y., in 1852, and studied at New York University and Columbia University. He was for a short time assistant professor of geology at Columbia University and became geologist in the U. S. Geological Survey in 1880. This position he subsequently retained. In 1892 he became professor of geology in the University of Michigan. Professor Russell was vice-president of the American Association in 1904 and was president of the American Geological Association at the time of his death.

Professor Gabriel Oltramare, formerly professor of mathematics at Geneva University, died on April 10, in his ninetieth year.

The House Committee on coinage, weights and measures on April 27 voted down a motion to report the Littauer bill establishing the metric system of weights and measures.

Dr. Nathan C. Schaeffer, president of the National Educational Association, writes from the executive committee in session at the Auditorium Hotel, Chicago, Ill., on April 28, "In view of the appalling calamity which has visited San Francisco it is impossible for the National Educational Association to hold its meeting this year in that city. After fully considering all the letters and telegrams which have been received from all parts of the United States, and after carefully weighing what is due the people of San Francisco, the executive committee, under the authority conferred upon it by the board of directors at its last meeting—the board of trustees, now in session, concurring—decides to postpone the annual convention of the National Educational Association for one year, to a place yet to be determined. They join in the hope that the association may meet in San Francisco as soon as feasible."

The annual meeting of the South African Association for the Advancement of Science will be held at Kimberley on July 9–14, under the presidency of Mr. G. F. Williams.

The eighty-ninth annual meeting of the Swiss Scientific Society will be held this year at St. Gall from July 29 to August 1, under the presidency of Dr. Ambühl.

Before the adjournment of the New York legislature Governor Higgins signed a bill creating the Hudson-Fulton Celebration Committee, and appropriating $25,000 to be expended by it for celebrating the tercentenary of the discovery of the Hudson River in 1609, and of the first use of steam in the navigation of the river by Robert Fulton in 1807.

The spring lectures given in the lecture hall of the museum building of the New York

Botanical Garden at Bronx Park on Saturday afternoons at 4:30 are as follows:

April 21. 'On the Correlation of Characters in Plants,' by Professor Hugo de Vries.

April 28. 'A Day at Hammarby, the Home of Linnæus,' by Dr. W. A. Murrill.

May 5. 'A Historical Review of the Study of Fossil Plants,' by Dr. Arthur Hollick.

May 12. 'A Glimpse at the Development of Botany in America,' by Professor L. M. Underwood.

May 19. 'The Effects of Radium on Plants,' by Dr. C. Stuart Gager.

May 26. 'Some Botanical Features of Porto Rico,' by Dr. Marshall A. Howe.

June 2. 'Orchids; their Botanical Features and Relation to Horticulture,' by Mr. G. V. Nash.

June 9. 'The Wild Vegetable Foods of the United States,' by Dr. H. H. Rusby.

June 16. 'The Origin and Adaptations of Desert Floras,' by Dr. D. T. MacDougal.

June 23. 'The Botanical Exploration of the West Indies,' by Dr. N. L. Britton.

THE general assembly of the state of Maryland, which has just adjourned, established a State Forestry Commission, composed of seven members. The governor, state comptroller, the president of the Johns Hopkins University and the president of the Maryland Agricultural College and the present State Geological Survey Commission comprise the new board, together with the state geologist and two members interested in forestry matters who are to be appointed by the governor. Professor Wm. Bullock Clark, state geologist, will become the executive officer of the board, which has the appointment of a state forester who will continue the forest survey already started by the State Geological Survey. The new board has the right to accept as gifts and to purchase lands for state forest reservations and also to appoint wardens in the several counties to protect the forests from fire and preserve the game.

ADDITIONAL legislation was obtained by the Maryland Geological Survey at the last session of the legislature, providing for the construction of a modern state road connecting Baltimore and Washington. The work on the road is to be taken up at once and completed within three years. The State Geological Survey has charge of all state road work and under the State Aid Road Act of 1904 has not completed its first year's operations, having contracted for about forty miles of modern roads last season which are being built at the joint expense of the state and counties. The amount available annually for this work amounts to $400,000.

A PRELIMINARY statement on the production of hydraulic cement in the United States during the calendar year 1905 has been issued by the United States Geological Survey. It shows that the total production of all kinds of hydraulic cement in 1905, including Portland, natural-rock, and slag or Puzzolan cements, was 40,894,308 barrels, valued at $36,012,189. This was an increase of 9,219,051 barrels, valued at $9,980,269, over the production of the previous year. Of the total amount of cement manufactured in the United States in 1905, 36,038,812 barrels were Portland cement, with a value of $33,326,523; 4,473,049 barrels were natural rock cement, valued at $2,413,052; and 382,447 barrels were slag or Puzzolan cement, valued at $272,614.

UNIVERSITY AND EDUCATIONAL NEWS.

MR. ANDREW CARNEGIE has given $100,000 to Lehigh University for the construction of a dormitory.

A PRESS dispatch states that the movement to increase the endowment of Victoria University, Toronto, by $300,000 is now practically completed. That amount has been raised all but $12,000, counting the $50,000 given by Mr. Andrew Carnegie. The latter donation was conditional upon the raising of an additional $50,000, but Chancellor Burwash is confident that there will be no trouble in fulfilling the condition. A new library building will be erected, capable of accommodating about 300 students, and with a stockroom capacity of 50,000 volumes.

ACCORDING to the N. Y. *Evening Post* Sir William McDonald, of Montreal, has given $55,000 for the purpose of erecting an extension to Prince of Wales College, Charlottetown, P. E. I. Additional facilities will be provided for teaching nature study, domestic

science and kindred subjects, and for training teachers.

We learn from English exchanges that the special board for mathematics has recommended important changes in the Cambridge mathematical tripos, which, they say, as at present constituted, exercises in several respects an unsatisfactory influence upon the course of study of the candidates. The board proposes that an examination of an elementary character be established to be called 'The Mathematical Tripos, Part I.' This examination would be taken by the better students at the end of the first year, and by others at the end of the second year. It would serve the two main purposes of relieving those who had passed it from the obligation of spending further time on the elementary parts of subjects contained in it, and of indicating a course and providing a test suitable to the needs of those students of engineering or physics who are willing and able to spend part of their time in acquiring such a knowledge of pure and applied mathematics as the examination will test. The present Part II. would be discontinued, the name 'The Mathematical Tripos, Part II.' being applied to an examination to be taken at the end of the third year. It is proposed that the list of successful candidates shall be arranged in the three classes of Wranglers, Senior Optimes, and Junior Optimes, the names in each class to be arranged alphabetically. The ablest students, being thus relieved from the necessity of competing for places in the examination, would be able to spend a considerable part of their time during the first three years on advanced work, whilst it is hoped that other students of somewhat less ability would be encouraged to spend part of their time in this manner.

An official report on the University of Paris by M. Tannery, abstracted in *The British Medical Journal*, shows that the total number of students in the university last year was 14,462, of whom 1,638 were foreigners. Of the whole number, 968 were women, and of these 513 were foreign. The teaching staff comprised 281 professors, *professeurs agrégés*, and lecturers. The number of students in the medical faculty was 3,482, being 93 less than in the previous year. The dean of the faculty is quoted as stating that 'he would rejoice if the diminution were still greater.' Of 109 foreign women who were students of the faculty. 98 were Russians. In the faculty of law there were 6,086 students, showing an increase of 1,289 as compared with the previous year. Of the law students, 231 were foreigners, mostly Roumanians, Egyptians and Russians. Of the representatives of the last-named nation, 29 were women.

Dr. Francis R. Lane, of Brooklyn, N. Y., has been appointed director of the Jacob Tome Institute, at Port Deposit, Md. He succeeds Dr. Abram W. Harris, who resigned to accept the presidency of the Northwestern University.

Professor Frank Thilly, who recently went from the University of Missouri to Princeton University as professor of philosophy, has been made professor of philosophy in the Sage School of Philosophy of Cornell University, succeeding Professor E. B. McGilvary, who was last year called to the University of Wisconsin.

The trustees of Wellesley College have recently made the following changes and new appointments in the department of botany: Dr. Margaret C. Ferguson was promoted from associate professor in charge to professor and head of the department. The title of Clara E. Cummings was changed from professor of botany to professor of cryptogamic botany. Mary C. Bliss was reappointed with the title of instructor and curator of the museum. H. S. Adams was reelected instructor and consulting landscape architect. The new appointments were: L. W. Riddle, instructor and curator of the phanerogamic herbarium; Mary F. Barrett, instructor; Caroline L. Allen, assistant.

Mr. R. R. Gates, M.A., who has just received the degree of B.Sc. from McGill University, and who during the past year has been engaged upon special cytological work with reference to an elucidation of the laws governing hybridization, has received an appointment to a senior fellowship in Chicago University.